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SOME NOTES ON THE EVOLUTION OF AIR DOCTRINE

By BERNARD BRODIE

I

MILITARY strategy is of all the human sciences at once the most ancient and the least developed. It could hardly be otherwise. Its votaries must be men of decision and action rather than of theory. Victory is the payoff, and is regarded as the most telling confirmation of correct judgment. There is no other science where judgments are tested in blood and answered in the servitude of the defeated, where the supreme authority is the leader who has won or can instill confidence that he will win.

Some modicum of theory there always had to be. But like much other military equipment, it had to be light in weight and easily packaged to be carried into the field. Thus, the strategic ideas which have from time to time evolved have no sooner gained acceptance than they have been stripped to their barest essentials and converted into maxims. Because the baggage that was stripped normally contained the justifications, the qualifications, and the instances of historical application or misapplication, the surviving maxim had to be accorded a substitute dignity and authority by treating it as an axiom, or, in latter-day parlance, a "principle."

The so-called "principles of war" have been derived from the work of a handful of theorists, most of them long since dead. Their specific contributions to living doctrine may not be widely known, because their works are seldom read, and the dimensions of the original thought may find but the dimmest reflection in the axiom which has stemmed from it. Nevertheless, by their ideas, however much those ideas have suffered in the transmission, these theorists have enjoyed in the most pragmatic and "practical" of professions a profound and awful authority.

Air power is too young to have among the theorists of its strategy more than one distinguished name, and he has carried all before him. The views of General Giulio Douhet would be

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worth study today even if air force thinking had progressed considerably beyond him and away from him, because he would still remain the first to have presented an integrated, coherent philosophy for the employment of air power. But the fact is that air strategists have moved very little beyond or away from him. American air strategists today may or may not acknowledge in Douhet's philosophy the origin of their present doctrine, but there can be no doubt about the resemblance between the two.[1]

We know, however, that Douhet's basic essay, "The Command of the Air," was read avidly in a condensed translation by leading American air officers during a crucial stage (the mid-thirties) in the development of the ideology of the U.S. Air Corps, and that it was warmly embraced by its readers.[2] We know that the development of air force doctrine followed Douhet not merely in a broad emphasis on strategic bombing, but also through some of the finer ramifications of his philosophy, such as a de-emphasis on fighters, whether for defense or for escort of bombers, and a corresponding emphasis on destroying the enemy air force at its bases. The relative disregard of fighter combat in air doctrine and planning was something that was distinctively Douhet's, and it is notable because it directly repudiated World War I experience. There are other comparably striking examples of identity—in fact, no important instances of difference.

It is of course obvious that Douhet's philosophy would not have been embraced so warmly—one might almost say, so uncritically—if it had not been so congenial. Ground force and naval officers have certainly not regarded it as self-evidently true, whether in detail or in broad outline. But for air force officers, its emphasis upon an independent mission for the air force and upon the sufficiency of that mission to achieve victory could hardly be unwelcome.

Douhet's success with one kind of military service is perhaps the most striking of any theorist in the annals of strategic thinking. The astonishing popularity of Mahan at the turn of the century comes to mind, but Mahan was after all a reinterpreter

[1] See the one-volume edition of Douhet works in English called *The Command of the Air* (New York, 1942), trans. by Dino Ferrari. The title of the volume is that of the first essay contained therein, but others of Douhet's essays are also included.

[2] See General H. H. Arnold, *Global Mission*, New York, 1949, p. 149.

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rather than a creator of strategic concepts. Douhet's strategic philosophy was by contrast wholly revolutionary. His success was quick and complete, not only in the American Air Force but in every air force which had the administrative and intellectual freedom to follow him, including the Royal Air Force of Great Britain,[3] and even the German Air Force. We would suspect that to be true of the latter organization from the way the Luftwaffe conducted itself in the Battle of Britain, but we also have the word to that effect of Generalleutnant Adolf Galland:

> Douhet's ideas met with a great deal of approval in Luftwaffe leading circles before the war. Although in the first phase of organization it was a fighter arm which stood in the foreground, in the second it was unequivocally the bomber. I still remember clearly a period when the talk was all of strategic bombers and one referred with something of pitying condescension to "home defense fighters."[4]

Of course, the German Air Force, like all continental air forces, was bound to the ground forces by the tremendous prestige and insistent demands of the latter. But the handling of the Luftwaffe in the Battle of Britain, which was supposed to prepare the way for a trans-Channel invasion (Operation Sea Lion), shows how restive it was under German army and navy demands. The airmen were determined to fight their own war, and to the best of their ability did so.

The only major air forces which seemed at the time of World War II not to be inoculated with Douhet's ideas were the Japanese and the Russian, and the reasons for these two exceptions are fairly obvious. In Japan, not only were the respective air forces under the dominance of the army and navy organizationally, as they were nominally in the United States, but the conservative army and navy leaders were in fact in control of

[3] In a published lecture, Marshal of the Royal Air Force Sir John Slessor scoffs at the idea that Douhet had had any special influence on RAF doctrine, but his subsequent remarks unwittingly confirm that influence. He incidentally attributes to Lord Stanley Baldwin the dictum, "The bomber will always get through," which, whether or not Baldwin ever actually voiced it, is certainly near the heart of the Douhet thesis. It is not remarkable that distinguished leaders should not know the source of their own ideas, since only professional scholars make a virtue of such knowledge. Slessor's lecture is reproduced in the *Journal of the Royal United Service Institutions*: "Air Power and the Future of War" XC, No. 595 (August 1954), pp. 343-58.

[4] "Defeat of the Luftwaffe: Fundamental Causes," *Air University Quarterly Review* (Maxwell Field, Ala.), VI, No. 1 (Spring 1953), p. 23.

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the government, which was certainly not true in the United States.

In the Soviet Union, the control of the armed services by the government was absolute and complete. There had been an enormous purge of leading military officers in the late thirties, and among those eliminated was Corps Commander (i.e., Lieutenant General) V. V. Khripin, Deputy Air Force Commander up to 1937 and chief Soviet air theoretician. The author of numerous articles on air power, he had also written the introduction to the Russian translation in 1935 of Douhet's *Command of the Air*. Shortly before the end of his career, he had been named First Commander of the short-lived "Army of Special Designation" (AON), a combined long-range bombing and airborne forces organization.[5]

In a purge environment, one does not have novel strategic ideas. After the liquidation of the generals, the leading military theorist in the Soviet Union was the same man who was also the leading social, political, cultural, and biological theorist—namely, Joseph Stalin. There is nothing like having a genius who regards all knowledge as his province. The Western world has not had such a one since Francis Bacon, who incidentally also had his say about strategy. And let us not be too contemptuous of the military comprehension of Stalin, who seems to have delved considerably into the work of Clausewitz. One of the major problems we are concerned with today is that of determining how much the new weapons, and the example of the USAF, have stimulated a reorientation of the air strategy thinking of the present Soviet regime, which now has to depend on assorted lesser geniuses than Stalin.

The contribution of Douhet which commands greatest respect is that he turned upside down the old trite military axiom, derived from Jomini, that "methods change but principles are unchanging." He insisted instead that a change in method so drastic as that forced by the introduction of the airplane must revolutionize all the so-called principles of war. It took a bold and original mind to conceive that the sacrosanct principles

[5] I am indebted to my colleague, Dr. Raymond L. Garthoff, for my information about Khripin.

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might be outmoded, and a strong and independent will to assert it. He not only asserted it but supported his arguments with remarkably firm and consistent logic. It would be well if we were capable today of the same kind of originality and boldness with respect to the new nuclear weapons.

But Douhet's most avid followers are not under the same compulsion as he to emphasize the violence of his break with the past. Military officers of all services seem to feel obliged to acknowledge allegiance to an ancient orthodoxy known as "the principles of war." The homage is usually toward a vague symbol, but there have also been attempts on the part of Douhet's adherents to mesh explicitly and in detail the new orthodoxy with the old. Douhet himself refused to justify his ideas according to whether they did or did not accord with some inherited gospel. He was much more interested in whether they accorded with the facts of life as he saw them. But perhaps his vision of the facts of life was not altogether uninfluenced by his training. At any rate, although he was too proud of his intellectual independence to appeal to the authority of the old principles where they happened to implement his own views, the fact is that they often did. Thus, the controversy over the proper role of air power has often, on its more intellectual fringes, revolved around the question whether the Douhet thesis, or, more loosely, the emphasis on strategic bombing, does or does not conform to the tried-and-true, "enduring" principles of war. On occasion the argument has even proceeded to exegesis of venerated authorities like Clausewitz, who after all has been dead for a century and a quarter.[6]

[6] See especially Captain Robert H. McDonnell, "Clausewitz and Strategic Bombing," *Air University Quarterly Review*, VI, No. 1 (Spring 1953), pp. 43-54. This article is a reply to the book by Admiral Sir Gerald Dickens, *Bombing and Strategy—The Fallacy of Total War*, in which Admiral Dickens argues that strategic bombing offends against the Clausewitzian doctrine "that the subjugation of an enemy is best accomplished by defeating its armed forces in battle." Replying to this and like objections, Captain McDonnell asserts that what is needed is "a closer examination of Clausewitz' principles"! For a more general effort to equate air force doctrine with the traditional principles of war, see also Colonel Dale O. Smith and Major General John D. Barker, "Air Power Indivisible," *AUQR*, IV, No. 2 (Fall 1950), p. 5.

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II

What then are these ancient teachings to which appeals are so constantly made? More important, from whence do they derive such commanding authority? We are here concerned with a body of ideas or axioms to which in our own time literally millions of lives have been sacrificed, and on the basis of which within the last decade great battles have been organized and fought. More to the point, we are concerned with a heritage of thought which even today dominates decisions on which the life or death of our nation may well hinge.

In the main, the maxims or axioms which we call "principles of war" are simply common-sense propositions, most of which apply to all sorts of pursuits besides war. If a man wants to win a fair and virtuous maiden, he must first make up his mind what he wants of the girl, that is, the principle of the objective, and must then practice the principles of concentration, of pursuit, of economy of force, and certainly of deception.

Let us acknowledge that common-sense principles are valuable precisely because they represent common sense, and are valuable only so long as they are compatible with common sense. There have been all too many examples in recent war of a slavish devotion to the so-called principles of war offending against common sense. One might say that slavish devotion to any body of maxims is likely to have that result.[7] In any case, the low intellectual estate to which these maxims have fallen today does not decently reflect their origin.

Karl von Clausewitz is the first great figure in what might be called modern strategy, just as Adam Smith is the first great figure in modern economics—to mention a science which is in many respects remarkably analogous. But unlike Smith, whose *Wealth of Nations* proved to be only the headwaters of a mighty and still expanding river of thought to which many great talents have contributed, Clausewitz is also, except for his lesser though impressive contemporary, Antoine Henri Jomini, almost the last great figure in his field. Others may also be worthy of honors

[7] For a more extended discussion of the relevance and irrelevance of strategic principles, see my "Strategy as a Science," *World Politics*, I, No. 4 (July 1949), pp. 467-88.

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and of notice, but they do not challenge his preeminence. And indeed it was very difficult to be original in this field after Clausewitz.

Not until two-thirds of a century later does anyone appear of anything like comparable stature, and Alfred T. Mahan, by confining himself to naval strategy, put himself into a rather more limited context than did Clausewitz. After Mahan we come to the unique name of Douhet, that is, unique in a separate and new field of strategy.

We are of course skipping over the names of some writers, not many, who would have to be considered in any history of strategic thought. Some of these showed real originality, and others are more important for their influence on their times than for originality or incisiveness. One of the latter is Ferdinand Foch, whom we shall discuss presently.

It is interesting to note that Clausewitz, who was certainly the most profound as well as systematic thinker on war who has yet appeared, specifically rejected the idea that there could be such things as principles or rules. One does indeed find discussed at considerable length in Clausewitz, as in Jomini, most of the basic ideas later to be exalted to the status of principles. But what makes large portions of Jomini and especially of Clausewitz come alive today in the reading is not the elucidation of basic ideas or principles but rather the wisdom that these two thinkers, one profound and the other incisive and eminently practical, brought to their discussion of these ideas. This wisdom is reflected in a flexibility and breadth of comprehension that makes Clausewitz dwell as tellingly on the qualifications and historical exceptions to the basic ideas he is promulgating as he does on those ideas themselves, though of course at lesser length.

Another respect in which the wisdom of Clausewitz is manifested concerns a subject in which his contribution is not merely distinctive but unique. No other theorizer on military strategy, with the possible exception of Mahan, has devoted anything like comparable attention and careful thought to the relationship between war strategy and national policy. Clausewitz' contemporaries, notably Jomini, took the dependence of strategy upon policy so completely for granted that they thought it worth little

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mention, whereas those who are more nearly our own contemporaries, notably Douhet, lost the point entirely or denied it.

In this regard more than in any other, Clausewitz has had not only the first word, but also practically the last. And in this respect as in all others, the fruits of his brooding intelligence have been not transmitted, but rather catalogued in the form of capsular quotations taken out of context. It is especially ironical that some of the very quotations which are often cited to prove that he was the prophet of total or absolute war are wrenched from a chapter in which he specifically insists that "war is never an isolated act" and that military aim and method must always defer to the political object.

Clausewitz is especially subject to such misinterpretation because of his subservience to the method of the contemporary German philosopher Hegel, whom he apparently studied with great reverence. Thus, after vigorously building up a case for war being in theory subject to no limitations of violence, he goes on to develop with equal vigor the point that in practice there must be many qualifications to the theoretical absolute,[8] which of course reflects Hegel's well-known method of presenting the thesis, the antithesis, and then the synthesis. This method, plus the natural inclination of a searching mind to feel all round the subject, makes Clausewitz amenable to being quoted on whatever side of an issue one desires, and he has been amply abused in this fashion. Moreover, he is of all the noteworthy writers on strategy the least susceptible to condensation. The efforts in that direction have almost invariably condensed out the wisdom and left the clichés, or rather what have since become clichés.

Let us now consider Ferdinand Foch, not because he fits anything like the same frame that Clausewitz does, but because he represents the more recent trend in strategic thought. He reflects on the one side something against which Douhet reacted violently, and on the other and greater side something that Douhet absorbed without question. Foch acknowledged himself to be a

[8] This entire exposition is contained within the opening chapter of his *On War*, though it is of course amplified throughout the book. The best and most available English translation of *On War* is that by O. J. Matthijs Jolles in the Modern Library edition.

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fervent admirer of Clausewitz; but where Clausewitz was the philosopher, wrestling with conflicting insights into contradictory truths, Foch was the teacher, determined to inculcate and indoctrinate. His object was not to explore but to explain, which meant inevitably to simplify and to exaggerate.

It must be said that on the battlefields of World War I he unlearned more quickly than did most of his colleagues and pupils some of the notions which as a writer and as a lecturer at the French War College, of which he was also for a time Commandant, he had been so instrumental in promulgating. It took real nobility as well as insight for him to say, as he is reported to have said to his staff following the disastrous French offensive at the frontiers during the opening weeks of the war: "Gentlemen, it remains for you to forget what you have learned and for me to do the contrary to what I have taught you." How deeply this conversion went is another question.

We are here interested only in the two main points in which Foch differed from his great predecessor of a century earlier and which are also meaningful for an understanding of the context in which Douhet wrote. First is the apotheosis of the offensive, which Foch and his followers pushed to the ultimate extreme; and second is the almost unconscious development of the conception that war is an end in itself pursued for its own objectives.[9] Both these points remain of overriding importance today, and explain much that would otherwise be inexplicable about patterns of military thought.

Foch was hypnotized by the offensive for morale reasons. Victory or defeat was to him not a physical fact but an attitude. One surrendered not because one was defeated, but because one felt defeated. Whatever the material costs, the posture of the attacker gave him the feeling of being the conqueror. Douhet,

[9] See especially Ferdinand Foch, *The Principles of War*, trans. by de Morinni, New York, 1918. By one of those odd quirks of distortion that result from the handing-down of isolated quotations, Foch is often quoted in the literature today as having asked the question, "De quoi s'agit-il?"—"What is it all about?"—usually interpreted as showing his profound concern with the object of war. Another quoted remark of his is "War is not the supreme aim, because after war there is peace." See, for example, Sir John Slessor, *Strategy for the West*, New York, 1954, p. 9. In such quotations from Foch, it is always necessary to ask whether the statement was made before, during, or after World War I.

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who wrote after World War I, had reason enough to consider this attitude preposterous, and his rejection of the ground offensive is one of the basic elements of his philosophy. He nevertheless exalted the offensive for aerial warfare, purportedly for strictly technical and physical reasons.

Clausewitz and Jomini had acknowledged that the winning of a favorable decision requires that one ultimately go over to the offensive and be successful in it. But Clausewitz had nevertheless reasoned that the defensive was the "stronger form of war," as evidenced by the fact that it was normally and appropriately resorted to by the weaker side, and, since he carefully distinguished between a defensive attitude and passivity, he insisted that the advantages of initiative and surprise did not necessarily accrue entirely to the attacker. He admitted that an army on the attack derives from the act of attacking a feeling of superiority which stimulates its courage, but "the feeling very soon merges into the more general and more powerful one which is imparted to an army by victory or defeat, by the talent or incapacity of its general." Jomini agreed with him so far as the tactics of battle were concerned. Waterloo was hard to forget. But he did insist that to be on the offensive strategically was always an advantage.

The school of which Foch was the intellectual leader had no patience with even those mild qualifications of the merit of the offensive which Jomini had seen fit to express. Its members exalted the offensive in all its forms, tactical as well as strategic, and refused to be confined by any considerations of circumstance. It was the favored pupil and protégé of Foch, Colonel de Grandmaison, who said: "In the offensive, imprudence is the best safeguard. The least caution destroys all its efficacy." Theirs was the religion of *l'offensive brutale et à outrance*.

Did no one admonish them to consider the machine gun? Possibly so, but it would have done no good. Foch did not deny that on the battlefield the attacker might lose more men than the defending opponent. Since his scholarship was always at the service of his convictions, he pointed to several historic battles in which, as he put it, "the decimated troops beat the decimating troops." Incidentally, if one wants to see how military history can be prostituted to the service of doctrine, it is a good exercise

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to check Foch's account of Napoleon's conduct at the battle of Wagram with any of the standard accounts of that battle.[10] This is only one case out of many which suggest that when one hears the phrase "history proves" one should get ready for bad history and worse logic.

Our comment above that Foch and his generation had slipped unconsciously into an assumption that victory is an end in itself cannot be implemented by quoting specific statements. After all, a statement to that effect would be too obviously absurd. One sees it simply in the attitude which pervades all of Foch's pre-World War I writings and utterances, and especially in the exaltation of the offensive regardless of cost. And in this, we must remember, Foch was only representative of his time. The British military leaders of World War I, who seem not to have read Foch at all and certainly did not attend his classes, clearly shared his convictions.[11]

We see in the ideas and attitudes just described one important explanation of the greatest disaster of modern times—World War I. World War I contains lessons which are in many respects more relevant to us today than the more recent war, which was after all only the most prominent of a long chain of dismal and catastrophic events which have year by year added proof of how hollow was that victory gained at such terrible price in 1918.

The soldiers of 1914 were not responsible for World War I, though some of their doctrines concerning mobilization clearly made it more difficult to prevent. The war would in any case have been costly, but certainly the doctrines that ruled the Western Allies and especially the French general staff made it far more costly than it need have been. Any war inevitably means killing and great loss of life, but World War I suggests that it makes a good deal of difference how it is fought. The generals of both sides were imbued with the idea that "war contains its own

[10] Foch, *op.cit.*, p. 322.

[11] Field Marshal Sir Douglas Haig, when asked by the Prime Minister in October 1918 what he thought the terms of the imminent armistice should be, recommended *in victory* practically the same terms that two years earlier he had rejected as an unacceptable basis for a compromise peace. The difference was that in 1918 Germany had been "vanquished," a condition that he had several times in his diary specified as being necessary to any peace. See *The Private Papers of Douglas Haig, 1914-1919*, ed. by Robert Black, London, 1952, pp. 104, 163f., 252, 294, 333f.

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logic," and that the single object was to win, forgetting that for the nation the word "win" must carry a meaning beyond the mere collapse of the enemy.

On the Allied side, especially among the French, there was in addition the religion of the offensive, which the Germans shared but with notably less fervor. The Allied captains were always preoccupied with what they were doing to the enemy, never with what they were doing to themselves. In a long series of dreadful and futile offensives designed only to "wear out the enemy," they sent to destruction those millions of young men whose existence was so necessary to give meaning to any victory. They were contemptuous of the politicians because the latter were concerned with casualty rates. And in their contempt they succeeded in imposing on themselves about twice the rate of casualties they imposed on the Germans.

We must always remember that Douhet offered his thesis as a superior alternative to the insensate bloodbaths of World War I which destroyed the real fruits of victory in the pursuit of the mere symbol of victory and which Douhet condemned in two of his most eloquent chapters.[12] And let us remember too that the soldiers of World War I were professionally competent men. It was their horizons rather than their skills which proved so disastrously limited. They were confident they had the right answers, and there is nothing in the stars that guarantees our own generation against comparable errors.

III

In our own time, we have witnessed the ultimate segregation from the other military arts of the pursuit of military history, which used to be the essence of theoretical study in strategy. Military history has now been turned over entirely to professional civilian historians, working either privately or under the employment of the services. The result has no doubt been better history, but also a profusion of volumes and monographs which the professional officer has neither the time, the incentive, nor the train-

[12] Chapters I and II of "The Probable Aspects of the War of the Future," contained in Douhet, *op. cit.*, pp. 148-77.

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ing to master. One natural result has been the divorcement of doctrine from any military experience other than that which has been intensely personal. This fact helps to explain the persistence of deep controversies in which each side implements its arguments with historical "facts" that, whether or not accurate in themselves, are almost always distorted in being wrested out of context.

World War II was certainly the best-recorded as well as the best-reported war in history. It is not the availability or lack of availability of the pertinent facts, but rather their volume, and the magnitude of the events they describe, which make it so difficult to form a just appraisal of any one campaign in the light of its total context. Naturally, the problem is made a good deal easier if one is emotionally committed to certain convictions that one wants to prove.

World War II brought Douhet's doctrines their first major test. Before we consider the results of that test, it might be well to summarize as briefly as possible the gist of his thought, which was far from being simply an advocacy of the kind of strategic bombing that was to develop during the war. His was a well-integrated philosophy, argued with exceptional internal consistency and built up with impressively rigorous logic. If anything was wrong with his system, the fault lay not with his reasoning but with his premises.[13]

Douhet rested his thesis fundamentally on what he deemed to be the contrasting situations on land and in the air. In land warfare, the defensive had proved itself (in World War I) vastly superior in intrinsic strength to the offensive, and this superiority, which resulted from improvements in firepower, was bound to progress further in the future. Ground fronts would therefore be static henceforward. In the air, on the other hand, the offensive was absolutely supreme. And since Douhet attributed an enormous destructive potential to a modest weight of bombs, and also believed civilian morale to be altogether incapable of facing up to bombing attack, he concluded that victory would be won,

[13] The best source in English for Douhet's ideas is of course the volume of his writings cited above. But a short substitute is my critical summary, "The Heritage of Douhet," *Air University Quarterly Review*, VI, No. 2 (Summer 1953), pp. 64-69, 121-27.

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and quickly, by the side that was able to get command of the air and attack the opponent's sources of strength at home. Command of the air was to be won not by fighting it out in the air, since interception was wasteful of resources and could not be relied upon, but by bombing the enemy air force at its bases. Air defense in all its forms, including interceptor aircraft, was futile and wasteful, both because it was vulnerable to bomber action and because the end would come too quickly to permit the attritive effects of air defense to develop. The speedy end also enabled one more perfectly to ignore the land front.

From these premises and propositions one could only deduce that the major portion by far of the nation's military resources should be concentrated in its air arm—the land and naval forces would need only enough to carry out their easily accomplished defensive function—and that the air arm should be comprised entirely of offensive forces dedicated to what we would now call strategic bombing. Douhet's operational slogan was: "Resist on the ground in order to mass [and attack] in the air."

And how did these theories fare in the great test? The answer to that question depends on the level of generality on which one seeks confirmation. If we disregard for the moment Douhet's over-all vision (which prompted the effort that was put into strategic bombing) and consider only his specific assertions (which considerably influenced the way the effort was expended), it is clear that in World War II Douhet was proved wrong on almost every important point he made.

Let us be clear that World War II was a fair test. It began fully eighteen years after his *Command of the Air* was first published and thus after considerable advances in the technology upon which Douhet had rested his thesis. And while none of the belligerent governments was anything like fully committed to his ideas, it is nevertheless a fact that the bomber fleets which ultimately took to the air were vastly greater, by several orders of magnitude, than those that Douhet thought would be sufficient to win a decision in a matter of days. The tonnage of bombs dropped within Germany alone were in wholly different realms of figures from those imagined by Douhet.

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Yet the evidence that the Allied bombing of Germany made a really critical contribution to the winning of the war is on the whole fairly equivocal.[14] Certainly it is clear that whatever important military results followed from the bombing did not come quickly, and, as we have seen, speed of decision in the air battle was vital to the integrity of the Douhet argument.

On more specific issues the tally of findings against Douhet is impressive. First, land fronts proved to be anything but static. Whole nations fell before strategic air attacks could fairly begin. Secondly, the fact that bombing could not bring anything like the swift returns that Douhet dreamed of and that vastly greater tonnages than he called for were necessary to bring any returns at all meant inevitably that defenses against air attack proved far more effective than he expected. Douhet did not deny that fighters could shoot down invading bombers, but he was able to postulate a situation where an attacking force could lose one-third of its strength on the first day of a war and then go on to win.[15] In World War II, where the bombing campaigns were trucking operations requiring repeated hauls by any one aircraft, attrition rates of 5 to 10 per cent could be serious to the attacker. The Battle of Britain resulted in an outright victory for the defense, and it does no good to say the attacking Germans "did not understand air power"; they were at the time quite literally following Douhet's precepts. The Allied assault on Germany resulted in a complete Allied victory in the air, but it was touch and go during more than one phase of the operation, and if the Germans had played their hand better—for example, by pushing rocket and jet fighter plane production—they might have made our losses prohibitive. Even the anti-aircraft gun, which Douhet so much despised, enjoyed the considerable respect of bomber crews.

The assault on Japan was another kind of case. As a test of Douhet's ideas it was vitiated in several ways, particularly by the fact that Japan was a defeated power—and recognized to be

[14] For a brief evaluation of our World War II bombing experience, as reconstructed mainly from the *U.S. Strategic Bombing Survey*, see my "Strategic Bombing: What It Can Do," *The Reporter* (August 15, 1950), pp. 28-31.

[15] In his fictional projection of a future war entitled "The War of 19—," contained in the above-cited volume, pp. 293-394.

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such by her military chiefs, especially of the navy—before our strategic bombing campaign was well begun. That is not to say that the bombing did not pay dividends; it certainly induced the Japanese leaders to acknowledge their defeat by surrender. But the origin of the defeat lay in other realms of action. In both Germany and Japan, the effects of bombing on civilian morale, and the effects of depressed morale on the strategic decisions of the leaders, turned out to be far less than Douhet predicted.

In one important respect, World War II was a better than fair test for Douhet, because there was for four years as static a land front along the Channel as could possibly be imagined, and this for reasons the exact opposite of those that Douhet presented. The surface stalemate in the west resulted not from the strength of the ground defensive, but from the swift and overwhelming offensive in 1940 of the German armies, which defeated the French and cast the British armies out of Europe. At the same time, the gigantic land and naval operations taking place in other parts of the world, especially eastern Europe, enormously assisted the effectiveness of the Allied strategic bombing in at least two ways. First, that fighting absorbed huge German resources which might otherwise have gone into the defense against our strategic bombing. Secondly, it put an enormous strain upon the German military economy, thereby making the German military posture far more sensitive to the effects of destruction from the air than it might otherwise have been.

If we were considering World War II as a test of strategic bombing *per se* rather than of Douhet's particular theories, some things would have to be said on the other side. First, we learned the importance of proper target selection and of the dependence of that selection on good intelligence. The two target systems that really paid dividends were, first, German oil production and associated chemical industries, and secondly, German internal transportation. Both systems were attacked late in the war, the latter almost at the very end. One of the reasons we attacked oil installations as late as we did—the big attacks did not start until May and June of 1944—was that we had all along assumed the Germans had oil stocks which in fact they did not have. Douhet did not prepare us for those lessons; he almost entirely neglected

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the importance of target selection and of associated intelligence.

Also, we learned after the war that the attack on enemy morale had been in the net a waste of bombs, which meant a waste of perhaps half the total tonnage dropped or more. The effects of the bombing on enemy morale were not trivial, and they are certainly a complicated thing to analyze; but it is abundantly clear that the deterioration of enemy morale resulting from bombing did not have important effects on military operations or on the outcome of the war. Finally, we learned that it was most important to overcome enemy air defenses, which meant, among other things, considerable dependence on escort by long-range fighters. Assuming we could apply those lessons to another strategic bombing campaign otherwise falling under World War II conditions, our strategic bombing might have far more positive results and certainly achieve them more quickly than it did in that war. That is not the same as saying that Douhet's theories would be confirmed, except in grossest outline.

Let us also remember that in World War II strategic bombing returned important indirect results besides its direct ones. And it was, after all, the only way we had prior to the landings on Italy and Normandy of striking at German power in Europe, and doing so while it was so heavily engaged against the Red Army.

Thus, one should be inclined to doubt the trite expression that generals always want to fight the next war in the way they fought the last one. If our air forces were guided by a consideration of their greater successes as against their lesser successes in World War II, they would today favor emphasizing tactical as against strategic bombing. There was hardly a more awe-inspiring success in World War II than the air operations in preparation for and attendant upon Operation Overlord, the landing on Normandy. The fact that the emphasis continues to be on strategic bombing is certainly not proved wrong by the experience of World War II, because the weapons and circumstances of today are vastly different from those of World War II; but it does recall Samuel Johnson's observation on the second marriage of a widower as representing "the triumph of faith over experience."

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IV

But now we must consider the effect of nuclear weapons of all sizes and in large numbers upon the Douhet philosophy. Surely, if it had not been for the atomic bomb, other advances in the technology of our time would have caused the Douhet thesis to die a natural death. Long-range bombing today would have to be done by jet bombers, which are much more costly to acquire and to operate than World War II types. We have every reason to suspect that even with jet bombers attrition rates in deep penetration sorties would be materially higher than they were in World War II. And in view of the very much longer ranges over which strategic bombing against the Soviet Union would have to be carried on, the HE bomb load per unit of sortie cost would fall absurdly low. Even if we could make every bomb dropped count for more than it did in World War II, because of better techniques and more knowledge about target systems, it is hardly possible that we could win a war against the Soviet Union predominantly, let alone exclusively, by strategic bombing with HE and incendiary bombs. We might in a test find that strategic bombing even as a totally ancillary activity simply did not pay, except perhaps as a demonstration. The same would be even more true of guided missiles, because of their lesser accuracy, if they lacked atomic warheads.

As we have pointed out, one critical cornerstone in Douhet's philosophy was his assumption of a very high yield of destructive effects from a small weight of bombs. This was proved wrong in World War II, but the fission bomb came along to rescue Douhet from that error and also to compensate for the much larger attrition rates that we should now have to expect. This was a rescue of a completely unforeseen kind, but a rescue nonetheless. It is unquestionably a triumph for Douhet that he was able to create a framework of strategic thought which is considered by many responsible airmen to fit the atomic age astonishingly well.

Let us go back to a period about three or four years ago. At that time our nuclear stockpile was much smaller than it is now, but what we had represented a monopoly. We did not have the necessary ground forces to make even a show of protecting

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Europe, and we clearly did not have the nuclear weapons necessary for large-scale tactical use if we were to carry out any strategic bombing missions at all. At that time, and for a limited period, we had no available alternative to a Douhet-type strategy. Whether that strategy would have been successful in a test is another matter. There is no point in speculating on it now. The real question is: does that situation still apply?

Certainly the enormous development of our military power since that time, and the rapid growth of our nuclear stockpile, suggest that now we may have alternatives. One point that Douhet overlooked might have been suggested to him by the World War I experience with naval blockade. The effectiveness of the naval blockade was greatly enhanced by the huge land battles that caused the Germans to consume so much of their military substance. No doubt Douhet overlooked it because he was so sure that the effects of the strategic bombing mission, and hence the end of the war itself, would come swiftly. But the relationship between strategic bombing and surface action has tended in the past to be reciprocal. The large-scale consumption of military commodities in ground action unquestionably makes the military economy more sensitive to hurt from the air.

And yet, developments have moved so rapidly that no doubt even this argument is already obsolete. With large numbers of atomic weapons, and especially with thermonuclear weapons, we do not have to concern ourselves with the sensitivity of the enemy war economy. Certainly we no longer have to concern ourselves with the finer points of target selection. General obliteration takes care of both sensitivity of the economy and discrimination concerning targets.

But now a quite different problem arises. The atomic bomb saved Douhet from oblivion. But it is no sooner appeared than it began to be spewed forth in such numbers and began to wax so great in size that it now threatens to go far beyond the stage that would redeem him from his errors. Perhaps it is threatening to destroy his philosophy with utter finality. For we can no longer bask in the comfort of monopoly. Have we really considered what that change means?

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Douhet's thesis rests on the argument that command of the air will be won very quickly, after which the winning side will have little to fear from the enemy's air power. And what happens during the brief struggle for command? Let us quote the relevant passage:

> Viewed in its true light, aerial warfare admits of no defense, only offense. *We must therefore resign ourselves to the offensives the enemy inflicts upon us, while striving to put all our resources to work to inflict even heavier ones upon him.* This is the basic principle which must govern the development of aerial warfare.[16]

This idea is really quite sound in an age of HE weapons, but how does it look in an age of thermonuclear ones? What are we resigning ourselves to? Is it a military pinprick, or is it total national disaster? And suppose we do achieve "command" in Douhet's sense. Douhet conceded that command could never be complete in the sense that the enemy was deprived of all capability of flying; but he insisted, quite rightly for his weapons assumptions, that a small capability would do the enemy little good. But suppose that small delivery capability is associated with H-bombs. And what happens when long-range ballistic missiles with thermonuclear warheads come into being? Then there will be no such thing as command of the air, unless—what seems unlikely—the side striking first can essentially wipe out with one blow the striking force of the other.

This brings us to what seems to be the crucial fallacy in the Douhet position for today's world. It is the unquestioning and almost unconscious assumption, which he shared with Foch and other World War I commanders, that war is an end in itself, ruled by a logic of its own, and fought for nothing outside itself. This idea naturally implies that every modern war must inevitably be a total war, which must now mean pretty nearly total destruction. There is nothing right or wrong but thinking makes it so, and if all our military leaders, and the enemy's too, are firmly wedded to such a conception, then of course it must be true—because they will make it true. The Korean War did not turn out that way, but for that very reason it seemed to baffle us completely.

So long as the view persists in high military and political circles

[16] Douhet, *op.cit.*, p. 55. The italics are Douhet's!

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that *any* war which brings the Soviet Union and the United States into direct and open conflict *must* be total, so long will preparatory measures be adopted which ensure that the opening of hostilities does in fact precipitate total war. It is obvious that one of the great inducements to the American leaders to keep Korea limited was precisely the desire to maintain a favorable posture in the event of a more direct challenge in a more important region.

We must therefore proceed to rethink some of the basic principles, which have become hazy since Clausewitz, connecting the waging of war with the political ends thereof, and to reconsider some of the prevalent axioms governing the conduct of military operations. What are suitable political objectives to be sought through military action in crisis situations, and what are suitable military measures for bringing them about? Above all, what are the available instrumentalities for assuring that military action does not proceed beyond the suitable? If our strategic air force is a retaliatory force, as is so often asserted, what kinds of action will it retaliate against?

We are now obliged to turn against Douhet the very same castigation which he so eloquently hurled against the French general staff of World War I. What did they think their nation was fighting for? Certainly not the destruction of its future. The difference between then and now is a difference in the magnitude of the disaster we have to consider. The capacity of the French general staff to sacrifice the resources of their nation through the obstinate application of a fanciful doctrine was after all limited. They could burn up only that manhood of a given age group among their citizens who were relatively sound of mind and limb. They could consume the commodities produced by their economy, but not the economy itself. The comparable power of the modern military planner, on the other hand, is for all practical purposes unlimited. He can guarantee a kind of war, which, because of the very exclusiveness of his preoccupation with what he is doing to the enemy, assures us of like destruction.

One cannot assert that the Douhet conception, which obviously still guides our thinking and decisions on air strategy, is clearly wrong for today's world. But it is legitimate and necessary to

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point out that vast and dire changes in circumstances are occurring before our very eyes, and that it is dangerous in the extreme to hold rigidly to the idea that all these changes merely implement and do not challenge a conception developed before these changes could be in the slightest degree foreseen. Douhet himself would not have done so. He was much too imaginative and original for that.

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MAX WEBER AND RUSSIA

By RICHARD PIPES

I

WHEN Max Weber published in 1906 two book-length studies of contemporary Russian politics, *Zur Lage der bürgerlichen Demokratie in Russland* (On the situation of bourgeois democracy in Russia), and *Russlands Übergang zum Scheinkonstitutionalismus* (Russia's transition to pseudo-constitutionalism), he was venturing upon a field of inquiry in which he had previously shown no interest, and which seemingly lay entirely outside the range of his professional qualifications. He was by this time widely known as an economic historian specializing in agrarian and financial problems, and in the methodology of the social sciences. True, two years earlier he had also departed from his specialty by writing on the relationship between capitalism and Protestantism, but in that instance his deviation had not been quite as radical, since these studies had concerned the economic implications of a religious movement. What could have induced him, so shortly after recuperation from a nervous breakdown which had incapacitated him for the better part of five years (1897-1902), to interrupt his academic routine, acquire a reading knowledge of Russian, and devote several months to the tedious perusal of the Russian daily press? Why did he, as his widow and biographer reports, "follow for months in breathless tension the Russian drama" of 1905?[1] Weber's interest in Russian politics, it is obvious, had deeper motives than mere fascination with current events; indeed, it was intimately connected with his two most vital concerns: the future of Germany, and the future of free society.

From the point of view of his political allegiance, Weber represented a not atypical amalgam of late nineteenth-century German nationalism and power-political inclinations, on the one hand, and democratic idealism, on the other. "I am a member of the bourgeoisie," he asserted proudly in his inauguration

[1] Marianne Weber, *Max Weber—Ein Lebensbild*, Tübingen, 1926, p. 342.

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speech at the University of Freiburg in 1895, "I feel myself as such, and I am raised on its views and ideals."[2] We have it on the authority of Karl Jaspers, his friend, that Weber remained faithful throughout his life to the notions of inalienable human rights and the dignity of man.[3] Weber himself never tired of reasserting his adherence to the principles of democracy. Side by side with these beliefs, Weber openly admitted his nationalist convictions: "I have always viewed politics solely from the national standpoint," he asserted shortly before his death, "not only foreign politics, but all politics."[4] His first public act, the speech at Freiburg, was a strongly worded warning of the "dangers" facing Germany from the infiltration of Slavic peasants into Germany's eastern provinces;[5] his last were his service as German delegate to Versailles and membership in the commission drafting the constitution of the Weimar Republic in 1919-1920. Between these years lay a quarter of a century of close personal identification with German politics, during which, for all his criticism of the Hohenzollern regime, Weber never once turned his back on his country, not even at times when by its activities it had violated his deepest humanitarian convictions.

Within the German context, these different sentiments were by no means incompatible. Weber's dualism was a reflection of the split personality of German liberalism, which throughout the nineteenth century found itself torn between its nationalism, which had induced it to cooperate with such exponents of power-politics as Bismarck, and its liberal goals in economics and internal politics, which had induced it to join the opposition. Criticize Bismarck as he might, Weber grew up in an atmosphere so thoroughly dominated by the ideas and personality of the Chancellor that he derived many, if not most, of the premises of his political theory from the lessons of Bismarckian politics. Above all, he derived from them the belief that power forms the essence of politics, defining politics as an interest "in power

[2] Max Weber, *Gesammelte Politische Schriften*, Munich, 1921, p. 26; henceforth referred to as *GPS*.
[3] Karl Jaspers, *Max Weber*, Oldenburg, 1932, p. 66.
[4] *Hilfe* (Berlin), November 9, 1916.
[5] *GPS*, pp. 8-30.

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distribution, power retention, and power shifting."[6] It is for this reason that an avowed liberal such as he could select as his closest political associate Friedrich Naumann—the theorist who, by formulating the concept of Germany's *Lebensraum* in Central Europe, had helped create one of the basic doctrines of future National Socialism.

Weber's nationalism was one reason for his interest in Russian politics. Russia concerned him as Germany's most powerful neighbor and competitor, whose internal development had a direct bearing upon the future of Germany. Another reason was contained in his philosophy of history, and particularly in his views on the tendency of Western politics.

In the course of his historical researches, Weber came to the conclusion that the main tendency of European history pointed in the direction of progressive rationalization of all aspects of organized human life; the characteristic feature of Western civilization, in his eyes, was the process of "disenchantment" (*Entzauberung*). This prospect frightened him, for he envisaged the likelihood of reason—once the liberator of man from the bonds of ignorance, superstition, and prejudice—becoming in turn man's new enslaver. A fully rationalized society would deprive man of his freedom and reduce him to the condition of a powerless cog in a virtually indestructible machine. Weber's dread of the ultimate triumph of the principle of rationality can be explained only by the fact that his faith in the omnipotence of reason was matched by an equally firm attachment to the ideals of freedom and individualism.

These philosophical considerations stimulated Weber's interest in Russia, because Russia appeared to him a country whose culture was only in its formative stage; it could, therefore, if its development were to proceed in a certain way, offset the growing tendency toward the suppression of human freedom which appeared to him the logical outcome of the course of European history. If his first interest was frankly subjective in its motivation, the latter was meant to be entirely dispassionate and free of national prejudices. One of the difficulties in analyzing Weber's

[6] Quoted by A. Mettler, *Max Weber und die philosophische Problematik in unserer Zeit*, Leipzig, 1934, p. 17.

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thought on political subjects lies in the fact that it operates on two distinct levels: at one time, Weber speaks as a German politician, interested in enhancing the power and prestige of his nation, and at another time as a detached scholar, viewing events *sub specie aeternitatis*. Both of these mutually exclusive attitudes are in evidence in his Russian studies, and unfortunately the lines separating them are not as clear as Weber, fully conscious of this ambivalence, might have desired. Apparently thought and life could not be as strictly separated as the Neo-Kantian school, by which Weber was strongly influenced, had presupposed.

From the political point of view, Weber divided society into three main groups: the group which holds the reins of power, the group which carries out the desires of those in power, and the subjects. The specific feature of all political bodies is the exercise of power. "All political formations are formations of violence."[7] Actual authority in every society, including one organized on democratic principles, rests in the hands of an elite: "The 'principle of the small number,' i.e., the superior political maneuverability of *small* leading groups, always dominates political activity. This 'Caesarian' stigma is ineradicable (in *mass states*)."[8] One of the most important criteria of a sound political organism is the extent to which this ruling elite is also economically ascendant. In a viable state, the material interests of the group which exercises political power coincide with the interests of the majority of the population and this interdependence is deeply ingrained in the consciousness of all the citizens. The group holding power must, in addition to economic power, also possess a well-developed power instinct, a *Machtwille*, which is not mechanically engendered by economic circumstances (as the Marxists would have it), but is slowly produced by zealous cultivation and continuous testing. It was very likely the lesson learned from the defeats suffered by the German liberals at the hands of the Junkers which led Weber to remark: "The ultimate substance of the *socio*-political problem is not the question of the *economic* status of those who are ruled, but much more so one

[7] Quoted in J. P. Mayer, *Max Weber and German Politics*, London [1944], p. 44.
[8] *GPS*, p. 167.

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of the *political* qualification of the *ruling and emergent* classes."[9] Political movements which lack these qualities are not likely to succeed, regardless of the number of their followers, the caliber of their leadership, and the soundness of their program.

To exercise its authority effectively, that is, to impose its will, the ruling group needs a certain measure of acquiescence on the part of the subjects. "A definite minimum of *wanting* to obey, that is, of *interest* (external and internal) in obeying, must exist in every genuine ruler-ruled relationship (*Herrschaftsverhältniss*)."[10] This acquiescence is essentially a matter of faith: "The basis of every authority, that is, of every subordination, is a *faith*: 'prestige'-faith in favor of the one or more who rule."[11] From the point of view of *type* of acquiescence which they secure, Weber divided all political systems into three principal categories: charismatic, in which the relationship between the rulers and subjects is based upon a non-rational "recognition" of the former by the latter; traditional, which gains obedience on the grounds of legitimacy and precedent; and, finally, rational—the modern European type—which obtains popular recognition through representation, referendum, and other legally formalized means. The rationally organized political system also uses for the purpose of securing mass approval of its actions the institution of the parliament:

> The modern parliaments are in the first place representative bodies of those who are *ruled* by means of the bureaucracy. A certain minimum of inner approval, at least on the part of the socially weighty layers of the subjects, is a prerequisite for the permanence of every, even the best organized, regime (*Herrschaft*). The parliaments are today the means of outwardly manifesting this minimum of acquiescence.[12]

It is not far-fetched to suppose that Weber's opinion of the limited functions of parliaments—like his whole scepticism concerning democratic legislation—was derived from the history of the Reichstag, which, in spite of its theoretically extensive legislative powers, had proved at all critical junctures in German history to be a malleable instrument in the hands of the crown

[9] *Ibid.*, p. 29.
[10] Max Weber, *Wirtschaft und Gesellschaft*, Tübingen, 1947, I, p. 122; henceforth referred to as *W&G*.
[11] *Ibid.*, p. 143.
[12] *GPS*, p. 158.

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and its ministers. Weber felt, however, that no matter how much parliaments were restricted in influencing the actual course of politics, they were indispensable to modern non-charismatic, non-traditional mass states as the only *continuous* means of eliciting popular approval of governmental actions; one might render parliaments harmless, as Bismarck had done, but one could not dispense with them altogether.[13]

In addition to a minimum of acquiescence on the part of their subjects, the rulers also need an apparatus through which to carry out their decisions. Weber's great contribution to political science lies in his thorough analysis of the administrative aspects of sovereignty, and in his emphasis on the political powers exercised in most societies by the ostensibly purely instrumental officialdom. Where most political thinkers, operating within the framework erected by the medieval jurists, saw the substance of the political problem in the relationship between the sovereign and his subjects, Weber, as a sociologist interested more in political practice than in legal theory, emphasized the third factor, the apparatus of administration. Each of the three types of political authority has its proper administrative system. In societies based on the charismatic principle, this function is performed by "men of trust": apostles, divinely inspired deputies of the leader, who convey his will to the subjects directly, without recourse to a permanent apparatus. Of course, such a system of administration is uncommon and by its very nature short-lived. In societies in which the prevalent type of authority is traditional, the officialdom—such as the hereditary nobility—consists of "servants" of the ruler; it lacks specific training for its duties and has no professional hierarchy. When such officials become subjected to a regular course of training and are transformed into a body of full-time salaried employees rated according to their skill and length of service, they become a bureaucracy in the proper sense of the word. The bureaucracy is the system of administration proper

[13] Cf. this with Bismarck's opinion: "I am in no way in favor of an absolutist government. I consider parliamentary *cooperation*—if properly practiced—necessary and useful, as much as I consider parliamentary *rule* harmful and impossible." (Italics added.) Speech in the Reichstag, 1884, quoted in Max Klemm, ed., *Was sagt Bismarck dazu?*, II, Berlin, 1924, p. 126. See also Otto Fürst von Bismarck, *Gedanken und Erinnerungen*, New York-Stuttgart, 1898, p. 412.

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to political authority based upon the rational principle, such as prevails in modern Europe.

If Weber devoted in his political writings a disproportionate amount of attention to the bureaucracy, it was because he considered it to be the central problem of modern politics. The emergence and spread of bureaucracies in the contemporary political machineries appeared to him an intrinsic part of the whole process of rationalization of Western life; it had its counterparts in industry, the army, political parties, and educational institutions. To Weber the state was an "enterprise" (*Betrieb*), and subject to the very same laws as any other enterprise. "The modern state, seen from the point of view of social science, is an 'enterprise,' exactly like a factory: this is precisely its historically specific quality."[14] Hence he stressed the administrative aspect of politics and tended to discount ideology: "We late men of the West have become sceptics," he once wrote. "Ideological systems shall no longer confuse us. Programs belong to the previous century."[15] The bureaucrat, as an administrator with a thorough knowledge of all the intricacies of his profession, was vastly superior from the point of view of efficiency to any other type of civil servant. Assuming the inevitability of rationalization of Western society, the complete bureaucratization of all social institutions was virtually inescapable:

> The future belongs to bureaucratization. . . . Where once the modern trained official rules, his power is virtually indestructible, because the whole organization of the most basic provisions of life is fashioned to suit his performance.[16]

Neither capitalism nor socialism was in a position to reverse this trend, since they too were succumbing to bureaucratization. Modern capitalism was thoroughly rationalized, and the business bureaucrats who administered it were not even remotely related to the individual entrepreneurs active during the early period of the capitalist system. The prospects for socialism were even more discouraging. The socialist state, by assuming ever greater social and economic responsibilities, would require a bureau-

[14] *GPS*, p. 140.

[15] Quoted by Otto Köllreutter, in "Die staatspolitischen Anschauungen Max Webers und Oswald Spenglers," *Zeitschrift für Politik*, XIV (1925), pp. 482-83.

[16] *GPS*, pp. 149-50.

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cratic apparatus much larger than the capitalist one. Furthermore, by destroying the capitalist entrepreneur who managed somehow to survive even under modern conditions, socialism would eliminate the *only* relatively efficacious rival of the bureaucrat: the only individual who knows his business better than the professional civil servant. Hence, Weber asserted, "growing 'socialization' today means unavoidably also growing bureaucratization."[17] Weber repeatedly criticized the Marxists for ignoring what to him seemed the central problem facing socialism: *who* will operate the nationalized enterprises? As for himself, he felt certain that this function would be assumed by the groups technically best-prepared for the task, that is, the bureaucracy, "from which nothing is further removed than [a sense of] solidarity with the proletariat."[18]

All these considerations led Weber to the pessimistic conclusion:

> . . . the edifice for the new bondage stands everywhere in readiness. . . . All the *economic* weather signs point in the direction of diminishing freedom. . . . "Against the current" of the material constellations are we "individualists" and party followers of "democratic" institutions. He who desires to be the weather vane of a "tendency of development" had better abandon these old-fashioned ideals as quickly as possible.[19]

The extent to which Weber dreaded the bureaucracy may be gauged from the fact that in one of his speeches on the political situation in Germany, he asserted that the American and French civil services were in the long run more beneficial to their countries than their German counterparts because they were more corruptible![20]

Only a dynamic, politically ambitious, and economically expanding social class could stem the tide of bureaucratic control, but such a class was missing from twentieth-century Europe. Political power in Europe was held by classes whose economic status was declining, while the proletariat, the only serious con-

[17] *Ibid.*, p. 141.

[18] Max Weber, *Der Sozialismus*, Vienna, 1918, p. 24; this essay is reprinted in Max Weber, *Gesammelte Aufsätze zur Soziologie und Sozialpolitik*, Tübingen, 1924, pp. 492-518.

[19] Max Weber, *Zur Lage der bürgerlichen Demokratie in Russland, Beilage, Archiv für Sozialwissenschaft und Sozialpolitik*, XXII (1906), pp. 347-48; henceforth referred to as *Zur Lage*.

[20] Mayer, *Max Weber*, p. 99.

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tender, lacked the "power instinct" (at least in Germany). The "dictatorship of the masses" which labor idealized pointed, in Weber's opinion, not to genuine popular rule, but to the dictatorship of individuals.[21] Europe, therefore, lacked a class able and willing to assume the responsibilities of political leadership; and, as a result, authority passed by default into the hands of the bureaucracy.

Under existing conditions, Weber thought that only a political system with a strong executive, preferably a monarch, and a strong parliament, both sharing control of the patronage, could keep the bureaucracy in check. The essence of the constitutional problem in the twentieth century was not the division of power between the legislative and executive organs of the state, much less the degree of popular control over the government, but the extent to which the executive and legislative could cooperate for the purpose of keeping at bay their common enemy, the civil service.

Surveying the European political scene at the beginning of the century from these premises, Weber had every reason for despair. Everything pointed to further decline in human freedom, and to the triumph of a ruthlessly efficient and practically indestructible—because rational—system of social organization.

If there was any hope for the preservation of liberty—in the broadest sense of the word—it lay outside Europe, in the United States and, above all, in Russia, where the conditions were still propitious for the emergence of new civilizations. It is difficult to overestimate the importance which Weber attached to the developments taking place in these two countries. The fate of Russia was of especial interest to him, partly because Russian culture was less "European" than that of America, and partly because developments in Russia were of more direct concern to Germany's international position. When the news of strikes and disorders spreading throughout the Russian Empire reached him, Weber immediately suspended his usual scholarly pursuits and immersed himself body and soul in the "Russian drama." At stake, he felt, was the fate of humanity itself: what was not gained for the cause of the individual and his freedoms during the period

[21] *GPS*, p. 391.

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of economic chaos in countries in which the economic system had not yet become fully crystallized might perhaps never be won "once the world has become economically 'full' and intellectually 'satiated.' . . . these are, in a certain sense, indeed, perhaps the 'last' opportunities for the construction of 'free' cultures 'from the ground up.' "[22] The second impetus for Weber's Russian studies thus stemmed directly from his philosophy of history. He reacted to the Russian Revolution of 1905 with the hope that it might be the harbinger of a new social and political order which would break the fetters strangling Western man and point the way to the rebirth of the free society.

II

Weber's first study dealing with Russia, *Zur Lage der bürgerlichen Demokratie in Russland,* which he completed in February 1906, was written with the assistance of Bogdan Kistiakovskii, a Russian liberal active in the *Soiuz Osvobozhdzheniia* (League of Liberation). Its historical background—whatever there is of it—is derived principally from Leroy-Beaulieu's *Empire of the Tsars and the Russians.* In this study Weber analyzed at length the social composition and programs of the principal Russian liberal groups, in order to ascertain the nature of Russian liberalism and the prospects for its success.

Weber proceeds from the assumption that nothing in Russia is "historical" except the peasant commune, the church, and the monarchy.[23] Consequently, he ignores the whole tradition of Russian liberal thought, the roots of which may be traced back well into the eighteenth century, and begins his account with 1903—the year when the middle-of-the-road elements working in the zemstvos united with the liberal intelligentsia to form the League of Liberation. His analysis starts with the events immediately preceding the establishment of the League, and then proceeds to the zemstvo movement, stressing particularly the relations between these twin pillars of liberalism and the two most dynamic

[22] *Zur Lage,* p. 349.

[23] Compare this opinion with Leroy-Beaulieu's: "The commune . . . is, properly speaking, and setting autocracy apart, the only indigenous institution, the only living tradition the Russian people can boast." A. Leroy-Beaulieu, *The Empire of the Tsars and the Russians,* London-New York, 1894, II, p. 2.

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classes of the population, the bourgeoisie and the peasantry. Weber's approach to the whole problem, his choice of evidence, and his emphasis are all conditioned by his political philosophy; he concentrates his attention on the "values" of Russian liberalism and on the extent to which it receives the backing of economically ascendant interests, on the assumption that such values and such support are necessary for the success of any political movement aspiring to power.

Weber's general conclusion to the main problems posed by his inquiry is negative: Russian liberalism lacks the support of groups materially interested in the realization of its goals, and its ideals are in many respects outdated.

The events of the 1905 Russian Revolution indicated to Weber that the bourgeoisie in Russia did not support the liberal forces, as represented by the zemstvos and the Kadet party. To begin with, the zemstvos were not genuinely middle-class institutions, that is, middle-class in the economic sense of the word: insofar as the bourgeoisie was represented in them at all, it was as a "general way of life and a level of education. . . . Viewed economically, the zemstvo liberals were on the whole 'non-interested parties,' hence bearers of a political and socio-political idealism. . . ."[24] The bourgeoisie, in the proper sense of the word, that is, the class of large industrialists and financiers, not only refused to assist these idealists, but actively opposed their cause. The break between the upper bourgeoisie and the zemstvo liberals, which had occurred early in the 1905 Revolution, was an outward manifestation of this rift. A far more encouraging aspect of zemstvo activity was to Weber the development of a zemstvo officialdom, the so-called "third element": the idealism and devotion of this official category, "which really lives 'among and with the people,' represent the ethically most encouraging and significant [phenomena] of today's Russia."[25] (Weber apparently had in mind the development of a grass-roots bureaucracy, capable of providing the anti-autocratic forces with the requisite administrative personnel.) Bourgeois support was also withheld from the other mainstay of Russian liberalism, the Kadet party.

[24] *Zur Lage*, p. 244.
[25] *Ibid.*, p. 245.

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The roster of the Kadet party contained no names of persons prominent in the industrial and financial circles of the country. The manufacturers and bankers preferred to join the conservative Octobrists, and sided on all decisive issues with the crown and bureaucracy against the liberals. As for the petty bourgeoisie, it was prevented from making common cause with the liberals by its anti-Semitism, enrolling instead in the ranks of the arch-reactionary Black Hundreds.

What was then left as a possible socio-economic basis for Russian liberalism? Only the peasantry. Some libertarian circles in Russia, notably the Socialist Revolutionaries, placed much hope on the emergence of Russian democracy on the foundation of peasant institutions, such as the commune. Weber considered this notion a sheer illusion, because he was convinced that such "romantic populism" would with time be undermined by capitalist forces, and give way to Marxism.[26] The peasants, in Weber's opinion, hindered rather than helped the development of Russian liberalism, by confronting it with an enormously complex economic problem which could not be solved without upsetting the entire social order. The peasants were basically anti-parliamentarian, desiring to deal directly with the Tsar and striving for the confiscation of land, and this striving the liberals must willy-nilly support in order to be consistent with their own egalitarian principles, although it was fundamentally anarchistic and economically retrogressive. Indeed, Weber saw no way at all in which the agrarian difficulties of Russia could be solved by democratic means: "The party which wants to carry out this [agrarian] reform in a *legal* manner, is not to be envied its task."[27] The Russian peasants would probably not ally themselves with the nobles to form a united anti-liberal front, as they had done in Germany; but they certainly would not cooperate with the liberals, either.

The cleavage between the liberals and the economically influential classes of Russian society—the general absence of a socio-economic basis in the liberal movement—was a shortcoming which pointed to the eventual failure of the whole cause of

[26] *Ibid.*, p. 346.
[27] *Ibid.*, p. 335.

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liberalism in Russia. Weber fervently cautioned against the optimism which encouraged many Russians to believe that regardless of the difficulties which democracy encountered in their country, its ultimate triumph was assured by the sheer force of historical progress. The spread of Western culture and capitalist economy did not, *ipso facto*, guarantee that Russia would also acquire the liberties which had accompanied their emergence in European history. In the first place, European liberty had been born in unique, perhaps unrepeatable, circumstances, at a time when both the intellectual and the material conditions for it were exceptionally propitious. It was a period of firm adherence to the principles of divine authority and the harmony of human interests—principles which since then had been undermined by Enlightenment and capitalism, respectively. It was also a time of overseas expansion and of unlimited opportunities for the individual capitalist entrepreneur. "These phases of development cannot be repeated in today's Russia, if only for 'ideal' reasons: the specifically bourgeois individualism is being overcome already within the classes of 'education and property,' and will certainly prove unable to win over the 'petty bourgeoisie.' "[28] In the second place, it was erroneous to ascribe to the advanced capitalism of the modern world, as practiced in the West, qualities of affinity with democracy and freedom, when, indeed, the question was: how, in the long run, are democracy and freedom possible under its rule?[29]

Although Weber was in general pessimistic about the prospects of Russian liberalism, he did not altogether discount its accomplishments. He called zemstvo liberalism "a splendid movement," comparing its role to that of the Frankfurt Parliament; it was a manifestation of "pure" liberalism. The zemstvos, he felt, could best serve the democratic cause by devoting themselves to the task of spreading among all the strata of Russian society the idea of inalienable human rights, in order to counteract the pernicious influence of both bureaucratism and Jacobinism. (By Jacobinism, Weber most likely meant Leninism, to which, in a note to the essay, he referred as the "Jacobin" tendency within the Rus-

[28] *Ibid.*, p. 280.
[29] *Ibid.*, p. 347.

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sian Social Democratic movement.[30]) Under modern conditions this was a vital task. With all the social forces conspiring against human liberty, the *desire* to be free was the last weapon available to man: democracy and freedom were possible only "where behind them stands continuously the determined *will* of a nation not to allow itself to be ruled like a flock of sheep."[31] Nor were all the sacrifices of the Revolution in vain. It was true that the monarchy had weathered the latest upheaval, and had retained the support of the army, the central bureaucracy, and some powerful social classes; as a matter of fact, Weber was convinced, "only an unlucky *European* war would definitively shatter the autocracy."[32] It was equally true that the efforts of the democratic forces had failed, and that Russia was now confronted with a regime in which the bureaucracy could dominate the country under the guise of a pseudo-constitutional system. But in the long run such a system could not prevail, and the country faced the prospect of continuous social upheavals.[33]

Several months after the appearance of his first essay on Russia, Weber published a second, longer study on the same subject: *Russia's Transition to Pseudo-constitutionalism*.[34] If in the first work he had approached Russian political life from the point of view of the development of liberal forces, now, having convinced himself of the liberals' weakness, he concentrated his attention on the tsarist regime and its machinery, for the purpose of evaluating the possibility of Russia's transformation into a Western parliamentary democracy from above, rather than from below. The tone of the second essay is cooler, more detached, more professional, than that of its predecessor. It was written by a man who, having been disappointed in his great expectations, approached the whole subject of Russian politics *sine ira et studio*.

The key to the behavior of the tsarist government during the 1905 Revolution, and the reason for the concessions granted in

[30] *Ibid.*, p. 281n.
[31] *Ibid.*, pp. 347-48.
[32] *Ibid.*, p. 338n.
[33] *Ibid.*, p. 353.
[34] Max Weber, *Russlands Übergang zum Scheinkonstitutionalismus, Beilage, Archiv für Sozialwissenschaft und Sozialpolitik*, XXIII, No. 1 (1906); henceforth referred to as *Scheinkonstitutionalismus*.

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the October Manifesto and promulgated in the Fundamental Laws, lay, according to Weber, in the foreign indebtedness of Russia:

> In order to understand the conduct of the Russian government, it is essential to keep in mind the fact that Russia is a debtor state. It is quite correct, as the reactionaries assert, that the "Jews" have extorted, smuggled, or fabricated the Russian Constitution; but naturally these Jews are not the terribly oppressed inhabitants of the Russian ghettos, but their partly ennobled cousins of the *haute finance* in Berlin and Paris, who are entrusted with control over the issues of Russian government stocks.[35]

For the sake of these foreign creditors, whom the disorders of 1905 had thrown into a state of panic, tsarism had to introduce the semblance of a constitutional regime and reach some sort of compromise with the domestic bourgeoisie. The main purpose of these measures was to create abroad the impression of internal stability and order, and in this manner to stimulate the flow of foreign loans and investments. In fact, however, the tsarist regime had not the slightest intention of granting the Russians any civic and political rights, and its main internal concern was the preservation of the extensive powers of the police bureaucracy. Thus, the government was compelled to conduct a dual policy: outwardly, it acted as a constitutional monarchy; internally, it retained the old system of arbitrary rule by the police.

The main subject of Weber's essay is the analysis of the 1906 Constitution from these premises. He approached the constitution from both the theoretical and the practical points of view, that is, how the new system was supposed to have worked, and how it actually did work. He analyzed at length the functions of the new ministerial body, the powers of the Russian legislature, the electoral law, and the role of the bureaucracy, stressing in each instance the limitations of the new arrangement, and the loopholes through which the anti-parliamentarian forces could reduce the opposition to impotence. Much of Weber's criticism was borne out by subsequent events: the inability of the Duma to assert positive leadership, the survival of the arbitrary rule of the police, the absence of a true cabinet. To Weber, the new system was not a genuine constitutional one, because it lacked the

[35] *Ibid.*, p. 170.

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division of authority between the chief executive and a political party in control of the parliament which he considered the essential feature of the constitutional system.[36] It was rather a "pseudo-constitutional" system. This term Weber applied not only to Russia, but also to contemporary Germany,[37] as descriptive of a regime in which the population, as represented by parties in the parliament, possessed not a share of political power, but only its appurtenances.

The arrangement of 1906 had, in Weber's opinion, two important and lasting effects: it increased the power of the Russian bureaucracy, and decreased that of the crown. The pre-1905 regime in Russia had been autocratic in name only; in fact, the country had been divided into numerous satrapies, which fought one another and the Tsar. The old system, by its very inefficiency, had created many avenues of escape from the clutches of the administration.[38] The October Manifesto had put an end to this state of affairs; it signified the demise of the old regime of overlapping authority, decentralized administrative apparatus, and competing institutions, and marked the definitive introduction of the centralized rule of the modern bureaucracy. Headed by the Prime Minister and the Council of Ministers—"that mighty trust of bureaucratic interests"—both entirely independent of the Duma, the bureaucracy now wedged itself between the monarch and the population. The Tsar retained only veto powers, and in all matters of administration became dependent on the ministerial body, which had gathered into its hands all the strands of effective political authority. The 1906 Constitution represented, in effect, "the definitive bureaucratical rationalization of the autocracy in the entire sphere of domestic politics."[39] Its net result was the *shift of power from the crown to the bureaucracy*. Had the Tsar permitted the creation of a genuine parliamentary system, he would have been able to retain more authority, because he would then have been in a position to compel the

[36] *W&G*, I, p. 173.
[37] Mayer, *Max Weber*, p. 47.
[38] This view is derived from Leroy-Beaulieu, *op.cit.*, II, pp. 77-81; see Weber's acknowledgment in *W&G*, II, p. 672.
[39] *Scheinkonstitutionalismus*, p. 228. See Weber's allusion to Bismarck's role in Germany, *W&G*, II, p. 672.

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bureaucracy to side with him against the legislature. By divesting the Duma of authority, he had ceded to the officials that political power which he had refused to yield to the population at large. In the long run, the principal loser was the Tsar:

> . . . as "flattery is homage paid by vice to virtue," so is the explicit codification of such a profoundly ungenuine pseudo-constitutionalism an equally profoundly degrading "obeisance of the idea of autocracy to the constitutional principle"; it harms, in the long run, not the respect due this principle, but the authority of the crown. . . .[40]

The 1906 constitutional arrangement, being backed only by the bureaucracy and the wealthiest elements of the bourgeoisie, and rejected by all the other classes of the population, would result in a conflict between the regime and society, and lead to chronic civil wars.

Weber's researches had thus convinced him that it was vain to look to Russia for new forms of civilization, and that his visions of the emergence there of "a free culture from the ground up"—although precisely *how* he had expected this to be done he had never made explicit—had been unfounded. In Russia, as elsewhere, power was passing into the hands of the bureaucracy. The 1905-1906 events in Russia indicated that this country had entered "on the path of the specifically European development."[41] This fact might have encouraged some friends of Russian freedom, but not Weber, because to him freedom, under modern conditions, was possible only *outside* European culture. Disappointed, Weber abandoned his studies of Russian politics. Henceforth, his views on Russia became progressively less influenced by cultural and philosophical considerations, and ever more colored by his German nationalism. While it is certainly unfair to accuse Weber of "Russophobia,"[42] it is nevertheless true that after 1906 he displayed a tendency to judge the internal and external policies of Russia from the point of view of their bearing on German political interests. This approach, in turn, led him—in political matters, at any rate—to assume an anti-Russian position.

[40] *Scheinkonstitutionalismus*, pp. 249-50.
[41] *Zur Lage*, p. 349.
[42] As is done by Mayer, *Max Weber*, p. 26.

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Weber spelled out the reasons for his change of attitude toward Russia in the course of a speech delivered in October 1916 on the subject, "Germany Among the European World Powers."[43] He had been critical all along of the foreign policies pursued by Wilhelm II, on the grounds that they were unrealistic and imposed upon Germany tasks which, owing to its peculiar geopolitical situation, it could not fulfill. In this speech Weber continued his criticism of the German regime and, in words as pointed as possible under the conditions of wartime censorship, indicated the disastrous consequences which the continuation of such policies could entail for the entire future of the German state. His main argument ran as follows: Germany is the only world power situated in the center of a continent and surrounded by other world powers. This unique position makes it necessary for Germany to have allies; it is the only state among those engaged in the conduct of world politics which cannot dispense with the assistance, or at least friendship, of other such states. In its quest for allies, Germany's choice lies between the West, as represented by England and France, and the East, as represented by Russia. Faced with this alternative, Germany must turn to the West. Russia is the natural enemy of Germany because: (1) Russia's population is constantly growing, while that of the Western countries is declining; (2) Russia is compelled by its land-starved peasantry to expand; and, finally, (3) Russia's interests are in conflict with those of Austria-Hungary and Turkey, upon whom Germany relies in its Balkan and Near Eastern activities. On these grounds, Weber advised the Germans to transcend their emotions and halt the campaign aimed at stirring up hatred of England. Nothing could be more disastrous than to alienate Britain and France beyond all possibility of reconciliation. Deprived of Western sympathies, postwar Germany would be left completely at the mercy of Russia.

It is characteristic of Weber's political thought that in these remarks, despite their pointedly anti-Russian conclusion, there was no dislike of Russia as such. Weber tried to keep moral and emotional elements out of his considerations, and to arrive at his

[43] "Deutschland unter den europäischen Weltmächten," *Hilfe* (Berlin), November 9, 1916.

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conclusions solely on the basis of cold calculation. What concerned him as an avowed *Realpolitiker* was the question: who could offer Germany more? His attitude toward Russia was determined entirely by this consideration.

The outbreak of the Revolution in Russia prompted Weber once more to turn his attention to the internal developments in that country. On April 26, 1917, he published in the journal *Hilfe*, edited by Friedrich Naumann, an article entitled "Russlands Übergang zur Scheindemokratie" (Russia's transition to pseudo-democracy), which was devoted to an analysis of the March Revolution.[44] The principal purpose of this somewhat hastily written essay was to indicate the possible impact of the Russian coup on Germany, and particularly on Germany's war effort.

Why, Weber inquired at the outset, did the Revolution of 1917 succeed whereas that of 1905 had failed? The answer lay in the behavior of the bourgeoisie, which controls the credit indispensable for the success of revolutions. In 1905, the middle class had supported the monarchy; in 1917, after another decade of rule by an arbitrary police and an irresponsible sovereign, it turned against the monarchy. Already during the war the most conservative elements of Russian society, driven to desperation by the Tsar's meddling and incompetence, had come out in favor of a parliamentary system. Had the bourgeoisie remained loyal to the regime, the Revolution of 1917 would probably also have been eventually suppressed. Only Stolypin, with his strong leadership, had saved the Tsar from meeting his fate earlier.

Weber considered the Provisional Government in the first month of its existence to be an uneasy condominium shared by the conservatively inclined bourgeoisie and its equally conservative allies: the officer corps, the state bureaucracy, and the domestic as well as foreign moneylenders, on the one hand, and the radical intelligentsia, representing the genuinely democratic forces, on the other. This condominium was made possible by a temporary community of interests of the two groups, but was

[44] The date June 24, given in Marianne Weber, *op.cit.*, p. 718, is erroneous. This article is reprinted in *GPS*, pp. 107-25.

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not likely to last, because their fundamental aspirations were quite divergent. The bourgeoisie had lent its support to the democratic forces only to get rid of an interfering monarch, not to introduce any far-reaching changes into the Russian political system. The interests of the bourgeoisie required the continuation of the war and the suppression of the democratic forces; the former to safeguard its investments and to secure foreign loans, the latter to prevent the powerful anti-proprietary sentiments of the masses of the Russian population from prevailing in the legislative organs of the state. The ideal of the middle classes was a "strong man," preferably a military dictator, capable of assuring these ends.

To retain control of the country until it was firmly in the saddle, the bourgeoisie needed the support of the radical intelligentsia, represented by the SR's and SD's, because the latter had at their disposal the means of transportation and communication such as the railroads and telegraphs, as well as contact with the masses through the rural bureaucracy, which was largely socialist in its sympathies. It was as a concession to these anti-monarchistic groups that the middle classes agreed to the complete removal of the Romanov dynasty, instead of replacing Nicholas II with a "citizen king" or a military dictator, as would have been more consistent with their own desires. The admission of the radicals into the cabinet also lent the new regime a revolutionary coloring without affecting in the least the hold of the conservatives on the machinery of the state.

The fatal weakness of the radicals was lack of both capital and credit, without which they could neither make a revolution of their own liking, nor shoulder the responsibilities of government. As a consequence they had to cooperate with the bourgeoisie, and content themselves with the miserable role of "fellow travelers" (*Mittläufer*) of an essentially anti-democratic regime.

A further cause of instability of the Provisional Government was the incompatibility of interests between the bourgeoisie and the most numerous social group in Russia, the peasantry. The peasants—both in and out of uniform—desired above all the prompt confiscation of large landed estates, and this in turn required the speedy termination of the war, with its concomitant,

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demobilization. Thus arose a social conflict which threatened the survival of the Provisional Government; whereas the middle classes desired the preservation of the status quo in matters of property and political structure, as well as the continuation of the war, the peasantry strove for radical alterations in the existing property relationships, the democratization of the government, and the conclusion of the war. Thwarted in its endeavors by the bourgeoisie, the peasantry could well seek violent solutions:

> These difficulties could be removed only by means of a social revolutionary *dictatorship* of many years' duration (under "social revolutionary" is to be understood not some kind of a savage, but simply a politician who pays no attention to the—altogether young in Russia—[concept of] "holiness" of private landed property). Whether personalities for this task are available, I do not know. But they can win lasting power only if *peace* is concluded most speedily. *Because only then would the peasants be within the country at all and stand available.*[45]

The democratic intelligentsia was thus caught in a vise between the bourgeoisie and the peasantry, each of which desired the establishment of a different kind of dictatorship. Weber thought it unlikely that the third major social force, the industrial proletariat, could serve democratic purposes, because its essentially anti-peasant interests made it dependent on the bourgeoisie; the workers could obtain greater concessions from a middle-class regime than from a peasant-dominated Constituent Assembly.

From all these observations, Weber concluded that as of mid-April 1917 Russia had not undergone a genuine revolution: "What has occurred so far is not a 'revolution' but a mere 'elimination' of an incompetent monarch."[46] As soon as the bourgeoisie had obtained sufficient credits abroad, it could be expected to eject the liberals from the administration, and put an end to all democratic sham. Unless Russia underwent a second coup, its basic policies would probably remain the same as they had been before 1917. Hence, the war against Russia must continue without letup, unslackened by any illusions about Russia's "revolution" or "democratization." Weber especially cautioned the leaders of German Social Democracy not to be misled by the

[45] *GPS*, p. 117. (Weber here indicates the possibility of a Socialist-Revolutionary, rather than Social-Democratic, dictatorship.)

[46] *Ibid.*, p. 120.

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appearances of Russia's change, and gave an early expression to the "stab in the back" myth:

> . . . [the] attitude of the Russian socialist leaders rests upon one fundamental assumption: namely, that now, when an army of Negroes, Ghurkas, and all the barbarian scum of the earth stands on our borders, half mad with fury, thirst for revenge, and the desire to lay waste our land—the German Social Democracy will still perhaps allow itself to play along with the swindle of the contemporary Russian Duma plutocracy, and will launch a moral attack from the rear on the German Army, which protects our land from the savages. . . . It is essential that the German working class *knows* the fact and the reason why for the time being there can be not the slightest doubt about the absence of any genuine "democracy" in Russia. With a *truly* democratic Russia, we could conclude an honorable peace at any time. With the present one, presumably *not*; because its rulers need the war for the sake of their power status.[47]

It is not difficult to perceive in these arguments a restatement of the contention which Weber had first made in 1906, when he had analyzed the prospects of Russian liberalism. Now, as then, he maintained that genuine democracy in Russia was unlikely because of the conflict between those who possessed the *means* for the establishment of democracy and those who, for material or idealistic reasons, had an *interest* in democracy.

In November 1917 the Provisional Government collapsed, and power passed into the hands of a small group of extreme radical intellectuals, leaning on part of the industrial proletariat and the army, and not on the peasantry, as Weber had anticipated. Weber discussed the new Bolshevik regime on two separate occasions: once in February 1918, in a newspaper article dealing with the contemporary European political situation, and then again in July 1918, in the course of a speech on socialism delivered in Vienna to a group of army officers.[48] The Bolshevik regime appeared to him a "pure military dictatorship" of corporals:

> It is pure nonsense to foster the impression that in back of it stand "class-conscious" proletarian masses of a Western European type. Behind it stands a *soldier* proletariat. This has its consequences. Whatever goals the Petersburg littérateurs may pursue, their power apparatus: the soldiers, awaits and demands above all *reward* and *loot*. This decides about everything. . . . The well-paid Red Guards have no interest

[47] *Ibid.*, p. 124.

[48] "Innere Lage und Aussenpolitik," *Frankfurter Zeitung*, February 3, 1918, reprinted in *GPS*, pp. 323-36; *Der Sozialismus*, Vienna, 1918.

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whatsoever in peace, which will deprive them of their earnings. Neither do the soldiers who, under the pretext of "liberation" of the Ukraine, Finland, and other regions, break in, and (as in Russia proper) impose contributions. The only authority in Russia established by means of a democratic—at least in form—election, the Constituent Assembly, has been destroyed by force. Not because of a major difference of views—its strongest party declared itself in favor of keeping the armistice and continuing the peace negotiations. But in order to prevent new providers from assisting other bodyguards, employees, and troop detachments with wages and loot. It is the purest militarism which exists at the present time anywhere. Everything else is, objectively, a swindle, whatever goals the management pretends to have, or perhaps really sincerely has.[49]

In his July speech, Weber elaborated further his misgivings about the stability and intentions of the Bolshevik regime. The main topic of the lecture was socialism. After characterizing Marxist socialism as a protest against the—to Weber, inevitable—alienation of the industrial worker from the means of production, Weber reiterated his scepticism concerning the ability of the proletariat to attain genuine control over the state and its economy:

Certainly: it is possible that in the course of a war, with the fabulous overturns which it brings, and as a result of the fate which the worker suffers, especially as a result of hunger, the mass of the proletariat . . . may, under the leadership of such intellectuals, seize power. . . . But I do not perceive the forces for the direction of production during peacetime either among the trade unionists or among the syndicalist intellectuals. The great experiment is now Russia.[50]

Recent information from Russia indicated, Weber continued, that piece wages had been brought back, and the old entrepreneurs, bureaucrats, and tsarist army officers had been rehired. "But one cannot permanently run the state machine and the economy in this fashion, and so far the experiment is not too encouraging."[51] Indeed, Weber expressed surprise that the Bolshevik regime had survived as long as it had, and ascribed its survival to the fact that the regime was less a socialist one than a "military dictatorship of corporals." Weber concluded his remarks about Soviet Russia with an assertion that Trotsky was mistaken in expecting the German Army to fall apart under the

[49] *GPS*, pp. 323-24.
[50] *Der Sozialismus*, pp. 29-30.
[51] *Ibid.*, p. 30.

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impact of Communist propaganda, because two-thirds of this army consisted of peasants and a considerable proportion of petty bourgeois, that is, of classes which would be only too glad to help subdue the industrial proletariat. But he warned his listeners not to underestimate the purely emotional appeal of socialism, and expressed approval of the Brest Litovsk negotiations, on the grounds that they exposed the real intentions of the Bolsheviks: "With fighters for a faith one cannot conclude peace—one can only render them harmless. . . ."[52]

The collapse of Germany in November 1918 came to Weber as no surprise; it was the fruit of years of irresponsible saber-rattling and adventurism, against which he had often warned his compatriots. He consoled himself, however, with the thought that perhaps not all the sacrifices which Germany had made in the course of the war had been made in vain:

> The self-discipline of truth naturally compels us to admit: the *world* political role of Germany is done for: the Anglo-Saxon world domination—"*Ah, c'est nous qui l'avons faite,*" as Thiers said to Bismarck of our unity. It *is* most unpleasant, but: something much worse—the *Russian* knout!—was turned away by *us*. This glory is ours. American world domination was as unavoidable as in antiquity that of Rome following the Punic War. It is to be hoped that it shall *remain* thus, that it shall not be shared with Russia. *This* is for me the goal of our future world policy, because the Russian danger is suppressed only for now, not forever.[53]

III

How, in the light of nearly half a century of historical retrospect, do the analyses and appraisals of Russian political developments made by a thinker widely recognized as the greatest of modern sociologists stand up against the test of history? It is true that Weber repeatedly denied any intention to predict. " 'History' cannot serve us as a prelude to the almost immediate present," he wrote at the beginning of his essay on the Russian constitution, "because we cannot know what will last. The question is one of retaining the essential and characteristic as they appear *at the moment.*" But, regardless of his disclaimers, Weber did in fact predict; indeed, the main purpose of his three princi-

[52] *Ibid.*, p. 31.
[53] Marianne Weber, *Max Weber*, p. 648.

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pal essays on Russia was to chart the probable future. It is not unfair, therefore, to confront his prognostications with the facts as they are known today, not so much perhaps to judge Weber's accomplishment as to obtain an understanding of the strengths and weaknesses of his political method.

The basic premises from which Weber approached Russian politics were derived from his conviction that Western civilization was moving inexorably toward ever greater rationalization of all aspects of organized life, and that this process led, in the realm of politics, to the transformation of the state into an "enterprise," with the attendant passage of all real power into the hands of the professional civil service. Weber found support for this thesis in the evolution of modern Germany, and he tended therefore to pattern his views on institutions and political processes on the German experience. It is there that we must look for the sources of his belief in the impotence of popular will, his extremely narrow conception of the scope and function of parliamentary institutions, his emphasis on the "manipulative," "elite" aspects of government, his contempt for non-professional politicians and exaggerated fear of all bureaucracy (the German *Beamtentum*), his insistence on the necessity of political leadership being combined with economic ascendancy, and, finally, his whole notion of politics as an "exercise of power." All these concepts Weber applied to Russian politics of the critical revolutionary periods of 1905-1906 and 1917-1918 with results which, on the whole, were not satisfactory.

The transition from the old decentralized, inefficient autocratic regime to the modern rational system of government by bureaucracy, which, by a process of deduction from more general principles, Weber came to regard as the main consequence of the 1906 Constitution, simply never occurred. On the contrary. During the final decade of its existence, the tsarist government became ever more chaotic and decentralized. Not a small measure of responsibility for this development must be assigned to the Tsar, Nicholas II, who, by his constant interference in administrative affairs, prevented able and loyal bureaucratic servants like Witte and Stolypin from providing the country with a more efficient government. The authority which the Tsar himself proved un-

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able or unwilling to exercise was arrogated not by the bureaucracy but by the Empress and her favorite, Rasputin; they, in turn, through the appointment of ineffective Prime Ministers like Goremykin and Stürmer, and through their constant intrigues, completed the disorganization of the old bureaucratic apparatus. Between 1906 and 1917, the Russian bureaucracy neither usurped the authority of the crown, nor attempted to do so.[54] It did its very best to serve the monarchy, but it became so thoroughly demoralized by the crown's irresponsible behavior that in March 1917, when the dynasty fell, it vanished from the political stage without so much as a token show of resistance. The record of Russian history indicates that between 1906 and 1917 the power of the monarchy remained undiminished, while that of the bureaucracy declined at an alarming rate—the very opposite of what Weber had anticipated.

Similar theoretical considerations caused Weber to misinterpret the two Revolutions of 1917. His political system did not allow for effective mass action, presupposing behind all political activity the manipulation of small groups operating in concert with the civil service and circles controlling money and credit. For this reason, in flagrant disregard of the evidence available to him at the time, Weber dismissed the March upheaval as a mere "swindle," engineered by the middle classes in conjunction with Western banking circles, and the November coup as another "swindle," this time perpetrated by the Russian Army. In fact, however, neither event was engineered, and neither was a "swindle." Viewed in historical retrospect, the year 1917 in Russia appears as a period of rapid dissolution of the political and social fabric of the country under the pressures of divergent popular forces such as the peasantry, the industrial proletariat, the army, the national minorities, and the intelligentsia, each striving for its own immediate interests. At no point was the Russian bourgeoisie, not to speak of Western capital, remotely in control of the situation. Even the Bolsheviks, who in the end succeeded in harnessing the destructive forces for their own ends,

[54] Good accounts of Russian politics during this period may be found in M. Florinsky, *The End of the Russian Empire*, New Haven, Conn., 1931, and Sir Bernard Pares, *The Fall of the Russian Monarchy*, London, 1939.

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did not so much direct the Revolution as allow themselves to be directed by it; it was Lenin's political genius to have made a matter of necessity appear as a matter of choice, and thus to have gained the appearance of leadership. By assuming *a priori* the indispensability of a trained and self-perpetuating bureaucracy in the modern state, Weber eliminated, for all practical purposes, the possibility of fundamental political changes in the modern world. This assumption led him, in the case of Russia, to deny a genuine change in the character and personnel of the Russian ruling group in March 1917, when in fact the change was fundamental and irrevocable. The same reason accounts for Weber's misinterpretation of the November Revolution. In this case, too, he found it inconceivable that a modern state could come into being as a result of a complete breach with the administrative apparatus of the previous regime,[55] and he explained seeming evidence to the contrary in terms of three possibilities: either the break was apparent rather than "real," or else it was "real" but only temporary, or, finally, if neither was the case, then the new state could not last. If Lenin, too, was proved wrong in going to the other extreme and completely rejecting the need for skilled officials ("Under socialism *all* will administer in turn and will quickly become accustomed to nobody administering"[56]), he was certainly closer to the truth than Weber, who saw in them the very keystone of every political organism. At any rate, Lenin succeeded in establishing a viable state with intellectuals and professional conspirators—the "littérateurs" upon whom Weber heaped such contempt—without any administrative experience whatsoever. Contrary to Weber's expectations, the Soviet state survived, and did so without falling into the clutches of the old tsarist bureaucracy.

It would certainly not be sound to appraise Weber's whole con-

[55] Cf. his statement in *W&G*, II, p. 670: "Under all the changes of rulers which had occurred in France since the First Empire, the political apparatus remained essentially the same. The reason for this is the fact that this apparatus—wherever it has control of the modern media of information and communications (the telegraph)—makes a 'revolution,' in the sense of a violent formation of entirely new political structures, an ever-greater impossibility, owing both to purely technical reasons and to its innerly rationalized structure; it has replaced 'revolutions' with *coups d'état*. . . ."

[56] V. I. Lenin, *Sochineniia*, 3rd ed., XXI, p. 452, quoted in E. H. Carr, *The Bolshevik Revolution*, I, New York, 1951, p. 243.

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ception of the modern state solely on the basis of his Russian studies, but it is quite clear that in the case of Russia his political theory had led him astray. His definition of the state as a factory-like "enterprise" caused him to overestimate grossly the importance of the technical aspects of government—the administrative apparatus, bureaucratic training and experience, and financial backing—and to underestimate or even ignore entirely those features of the state for which no parallels could be found in the rationally organized enterprise—tradition, ideology, public opinion, mass psychology. Indeed, his fascination with the techniques of government was so strong that he not only rejected the possibility of revolutions in the twentieth century, but questioned the feasibility of a genuine democratic system.

But apart from the ultimate merits or demerits of Weber's political theory, it may be well to inquire how sound he was in imposing upon the day-by-day political developments of a society with a rich and old history concepts derived from the experience of other societies. The crux of the difficulty seems to lie in Weber's assumption that Russia had no political history—that it was a *tabula rasa*—and consequently that its future political development could be fairly well charted on the basis of certain rational criteria and parallels with other, historically more "advanced," countries. This reasoning is responsible for his otherwise inexplicable disregard of Russia's whole political tradition, and for his unfounded hope in Russia's ability to produce an entirely new civilization "from the ground up." But to treat Russia as if it had only come into being in the twentieth century was profoundly un-historical; such an attitude posited the existence of universal criteria of a "civilized" community—in Weber's case represented by the ideal-type of nineteenth-century Western European civilization—and ignored the historical experience that such concepts as "civilization" are relative. In fact, Russia had behind it nearly five hundred years of continuous statehood during which, despite frequent changes in the structure of its government, it had evolved certain specific political institutions and attitudes which required serious attention, no matter how imperfect they looked to a contemporary Western European.

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Weber's approach to Russia's past had many affinities with the "philosophical" historiography which prevailed in Europe until the late eighteenth century, when European thinkers, partly under the impact of the French Revolution, came to realize its inadequacies as an instrument of social analysis, and evolved the methods of modern historicism. Indeed, what is the essential difference between Weber's bold imposition of patterns derived from the study of Western, largely German, political developments upon Russian reality, and the readiness of a Diderot or a Voltaire to offer Russian rulers advice based on the study of everything but Russia's own experience? And how does Weber's initial faith in Russia's unlimited cultural potentialities differ from the hopes entertained by many eighteenth-century philosophers, such as Leibniz, that Russia and other countries outside the Western orbit could evolve a more rational society because they were "starting from the beginning"? Behind both attitudes lie the same static concept of history, the same deductive method, the same disregard for local traditions and stress on universal values, and the same tendency to judge progress in terms of rational criteria. In retrospect, it appears that Weber would have been on far safer ground had he made an earnest effort to study the whole tradition of Russian politics, and evolved his analytical framework for the study of Russia from this record. For one, this would have led him to pay less attention to the Russian bureaucracy as a political factor, and more to the intelligentsia. The categories employed in the analysis of a given nation should be primarily derived from that nation's own internal history; any other approach, no matter how exalted its motives, tends to impress upon events patterns which are alien to them, and thus to distort.

How acute and penetrating Weber could be whenever he approached events with an open mind, unencumbered by rigid theories, emerges clearly from his analysis of those aspects of Russian politics where—at least for a time—he considered a deviation from the general European pattern possible: in his early studies of Russian liberalism and parliamentarianism. Hoping against hope that Russia might develop a viable parliamentary

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democracy, whose cause in the West seemed to him doomed, Weber subjected to close scrutiny the record of Russian liberal forces and the structure and operations of the new Dumas, and arrived at conclusions which time, by and large, has justified. Weber had developed to an uncommon degree a sense of political realities: his understanding of the "true" meaning of events, his ability to see through legislative camouflage and ideological pretense, and to estimate the interests of various social groups, were indeed remarkable. Thus, he appraised correctly the shortcomings of the 1906 Constitution as a democratic instrument, the weaknesses and strengths of the principal groups involved in the struggle for power, and the unsoundness of the Provisional Government. One of the themes which runs like a red thread through his Russian studies: the Russian liberal movement could not succeed because it lacked a socio-economic interest base, also proved to have been sound. Rather than seek the weakness of Russian liberalism in the alleged insignificance of the middle class in Russia, as was more customary at the time, Weber sought it in the cleavage of the two groups involved in the liberal movement: the liberal intelligentsia and the middle class. He thus foresaw the fatal conflict within the middle-of-the-road forces which eventually, at the critical moment in 1917, helped bring about the collapse of Russian democracy, although he did ignore the role which the radical intelligentsia and the industrial proletariat—both ineffective in Germany—played in this process in Russia. In these investigations, Weber was aided by his social approach to politics; it provided him with all the advantages open to socialist analysis, without the drawbacks from which the latter usually suffers owing to a certain rigidity inherent in socialist doctrine. Unlike the majority of Russian liberals, who stressed the formal, legal aspect of political institutions, Weber placed the emphasis on the relationship between these institutions and the interests of the various strata of Russian society. He also derived much insight from utilization of the experience of German liberal democracy, for, as a matter of historical fact, the situation of that force in Russia bore more similarities to the situation facing liberalism and parliamentarianism in Germany than to

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the one prevailing in the Anglo-Saxon world, which served most Russian liberals as a model.[57]

This method and skill would probably have stood Weber in good stead during World War I, had it not been for the fact that by this time another factor, his German nationalism, began to interfere with his scholarship and to becloud his vision. This emotional factor played an important role in his appraisal of events in Russia during 1917 and 1918.

In the end, Weber's enormous erudition and acute sense of political realities could not counterbalance grave shortcomings of his method, and the picture of Russian developments which he presented was, despite some valuable insights and remarkable predictions, a distorted one. The reason is twofold: his overestimation of reason as a factor in politics, and his fundamentally philosophic, un-historical approach. Both these traits reveal the extent to which Weber was imbued with the spirit of the Enlightenment.

[57] The Russian Fundamental Laws of 1906 were deliberately patterned after the Prussian as well as the Japanese constitution, which the Russian ruling groups took as prototypes of regimes with a constitutional order but without parliamentary rule. See P. Miliukov, in P. Milioukov, C. Seignobos, and L. Eisenmann, *Histoire de Russie*, III, Paris, 1933, pp. 1123-24.

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CIVILIANS, SOLDIERS, AND AMERICAN MILITARY POLICY*

By WILLIAM T. R. FOX

INTENSIVE study of factors affecting the relative power position of states "in the long run"—location, human and material resources, level of economic development and rate of capital formation, national solidarity, etc.—is no substitute for consideration of certain other factors which operate in the short run and determine the extent to which a state's power potential is utilized to achieve specific foreign policy objectives. Wars are fought or threatened and diplomatic settlements or impasses reached in the short run. So are decisions affecting all these grave matters, and sometimes with long-range consequences.

The main link between a state's long-run power potential and its government's achievement of present-day policy objectives are decisions taken regarding the level and character of military preparedness. Power potential is not a complete substitute for fighting capacity, especially when wars may begin with thermonuclear attacks which destroy the potential or prevent its being mobilized in time to influence the outcome of the war. Nor is it a substitute for the particular kinds of fighting capacity most efficient in deterring and localizing war.

The literature of American government and of international relations is, however, scanty in its description and analysis of the process by which technical military considerations and civilian policy considerations are fused to determine how big and what kind of military apparatus the United States is to have and where and for what purposes it is to be used.[1] Fifteen years of protracted high-level mobilization and the consequent necessity of correlating military policy and foreign policy objectives are only beginning to have an impact on American political science research by focusing attention on this relatively neglected "short-run" factor.[2]

* A revision of a paper read at the American Political Science Association meeting, September 10, 1953.

[1] See, however, Arthur W. Macmahon, *Administration in Foreign Affairs*, University, Ala., 1953, ch. 1, "The Concert of Judgment," pp. 1-56.

[2] See my research note, "Civil-Military Relations Research: The SSRC Committee

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It is perhaps not surprising that a heightened concern with military problems and with the adjustment of military means to industrial potential, on the one hand, and to foreign policy objectives, on the other, should have been so long delayed. In an earlier period, when the chief-of-state was likely to be his own field-commander, as were Washington, Napoleon, and Wellington, when the conduct of war required rather uncomplicated technical skills, and when the training of the governing class was in any case adequate to the arts of war, the adjustment of military means to policy objectives was viewed simply as a problem of *sub*ordinating the military to the civil power.[3]

Technical advances have made the statesman increasingly dependent on the soldier for dispassionate technical advice. To the former danger that an overstrong military power might threaten constitutional liberties from within has been added the new and, for twentieth-century democracies, the far more real danger posed by an ineffective civilian leadership and an uninformed public opinion. Maintenance of a level of armaments adequate to protect internal liberties against external threats and the precise shaping of armed forces to the specific goals of foreign policy require that military considerations in national policy be understood and evaluated by responsible and informed civilians.

This was not always so. In the nineteenth century, the United States was practically incapable of jeopardizing its security by any degree of neglect of its armed services. Just as it needed no allies,

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and Its Research Survey," *World Politics*, VI, No. 2 (January 1954), pp. 278-88, for a discussion of recently developing research interest in this area, and for an inventory of research currently under way which was prepared by Bryce Wood. So many political scientists have gained practical insight into these problems by their work in the Bureau of the Budget, in the Legislative Reference Bureau, and on the staffs of Congressional committees, as well as in agencies of defense and foreign affairs, that this research interest is likely to grow.

Literature on civil-military relations in the United States is listed in two bibliographies: Pendleton Herring, ed., *Civil-Military Relations: Bibliographical Notes of Administrative Problems of Civilian Mobilization*, Chicago, Public Administration Service, 1940; and *Civil-Military Relations: An Annotated Bibliography, 1940-1952*, prepared under the direction of the Committee on Civil-Military Relations Research of the Social Science Research Council, New York, 1954. The more recent of these bibliographies lists some five hundred items, but few of the items are concerned with the correlation of diplomatic, military, and industrial mobilization policy or with the problem of Executive-Congressional relations in this area.

[3] It was in these terms that the authors of the *Federalist* papers viewed the problem. See, e.g., Nos. 8, 26-29.

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because the great powers of Europe kept a zealous watch over each other and because British seapower was bound to confer upon it the benefits of alliance in the course of protecting Britain's own interests, so it needed little military and naval power of its own—except that which was necessary to bring order to such lesser breeds beyond the law as might be found in the Wild West, in the Caribbean and even, one hundred years ago, in the Far Pacific.

The present generation of Americans has had to learn what Europeans have long known—that there is danger in not having allies as well as risks in having them, and that there is danger in not having an effective military machine responsive to the needs of the country for protecting itself and its way of life. They have had to learn this elementary fact of international life because the United States during the last fifty years has lost its costless, foolproof security position. It was not just a matter of eliminating five bad men with immoderate foreign policy aims—the Kaiser, Mussolini, Hitler, Tojo, and Stalin—who had happened to become important political figures. If it were, Americans could hope to go back to sleep, safe behind their Atlantic Ocean barrier, now that the last of these five has departed this earth.[4]

The United States cannot again count on a two-year period of non-participation in a new general war and on a further period of relative safety, after it goes to war, to mobilize and to arm a fighting force. It will be, as the nucleus of the anti-Soviet coalition, the prime target of Soviet military effort, and advances in military technology have made it a feasible target. The increasing range of planes, submarines, and guided missiles, and the possibility of wreaking havoc through fusion and fission weapons without landing troops, have brought the United States closer to Europe and Asia and those continents closer to the United States.[5] These

[4] Not even the isolationists today doubt the necessity of preventing any single hostile power from dominating the whole of Eurasia. What divides the isolationists and the internationalists is disagreement over the efficacy of various kinds of politico-military action in forestalling that eventuality. The isolationist tends to rely more heavily on strategic air power and less heavily on coalition military action. How the development of tactical atomic weapons and of thermonuclear weapons affects this debate is not yet clear.

[5] The annihilation of distance as a benign buffer between the New World and the intermittent general wars of the Old World will be completed before the 1950's have run their course if John J. McCloy's prediction is borne out that "both we and the

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and other advances have so increased the time between weapon blueprinting and mass production that critical military decisions have to be made before, and possibly many years before, an actual war crisis.[6] Finally, in recent decades new inventions in techniques of government and administration have made it both possible for our prospective enemies to allocate 20 to 50 per cent of their national income for military purposes, even in peace, and mandatory for ourselves to arm to whatever extent necessary to meet their threat.

The convergence of all these factors and the heightening of their significance by the knowledge that a great power of potentially unlimited expansionist design has developed its own arms in a massive and threatening way mean that American foreign policy and military policy cannot again be divorced, even in peacetime. In support of a world-wide network of foreign policy commitments, the defense budget has grown to a point not imaginable even fifteen years ago. With the growth of the armed forces, there has been a great increase in the prestige of American military leaders and in dependence on the advice of these leaders, upon whose accuracy the United States stakes its national survival. It would be not only fruitless but wrong-headed to deplore this growth of military influence or to seek to block the access of military men to the civilian heads of the government.

It does, however, pose a problem. To what extent has the re-

Russians . . . in, say, five years . . . will have developed guided missiles to such a degree that they will be able to reach from continent to continent" ("Ten Years from Now," *Atlantic Monthly*, CXCIII, No. 6 [June 1954], p. 27).

[6] Even before World War II, the United States Army Chief of Staff declared: "What transpires on prospective battlefields is influenced vitally years before in the Councils of the Staff and in the legislative halls of Congress. Time is the only thing that may be irrevocably lost. . . . The sums appropriated this year will not be fully transformed into military power for two years" (General Malin Craig, United States Army Chief of Staff, June 30, 1939, quoted in Mark S. Watson, *Chief of Staff: Prewar Plans and Preparations*, Washington, D.C., Historical Division, Department of the Army, 1950, p. 30).

One "weapon" for deterring an atomic blitz, for maintaining the recuperative capacity for ultimate victory if the blitz should occur and if the retaliatory strike of strategic air power should not prove decisive, and for mitigating the human consequences of such war is the reconstruction of our great cities to make them less attractive targets. While the other weapons in the weapons system of continental defense take several years to develop and produce, decades may be required for urban reconstruction. Lloyd V. Berkner believes that "the gradual redesign and rearrangement of our cities in recognition of this new atomic environment can so reduce their vulnerability over a half-century or so that the need for excessive perfection in our military defenses can be substantially relaxed" ("Continental Defense," *Current History*, XXVI, No. 153 [May 1954], p. 262).

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sponsible civilian leadership compensated for the enlarged role of the military man in the making of high policy by an increased capacity to understand and evaluate technical military recommendations in the light of the non-military considerations of which the civilian heads of the state must also take account?

The unfortunate series of events which has impaired the Department of State's prestige has perhaps made it more difficult for some of these non-military considerations to influence national security policy. President Roosevelt by-passed the State Department during World War II and dealt directly with his military leaders and with other heads of states;[7] the State Department in the Truman administration was the object of constant, frequently irresponsible, and sometimes scurrilous attack; and in the Eisenhower administration it has been harassed by leading members of the President's own party. These successive traumatic experiences of the Department of State make the reconciliation of civilian and military considerations appear especially difficult. They obscure the substantial progress that has been made in developing institutions and personnel capable of integrating military and civilian considerations in the national security field.

The creation of the National Security Council, of a central agency for defense mobilization, and, in the Department of Defense, of a special office for dealing with joint State-Defense problems—all these show that the machinery of government has been strikingly altered to achieve coordination inside the executive branch.[8] It is possible that the National Security Council will develop into a powerful Cabinet-level committee which, by determining national security policy apart from other questions of national policy, achieves this coordination at the cost of fragmenting executive authority. It would be more in accord with the tradition of American government for it to remain in fact what it is in form, an advisory body to a chief executive who is constitutionally and politically responsible for all national policy,

[7] Cf. Cordell Hull, *Memoirs*, New York, 1948, I, pp. 195, 200; II, pp. 1109-11.

[8] The wartime informal meetings at the Secretary or Undersecretary level of top officials of the State, War, and Navy Departments and the creation at a lower level of the State-War-Navy Coordinating Committee, with a more restricted field of activity, represent earlier efforts to coordinate foreign and military policy. A comparable effort to reconcile military and naval (and air) policy resulted in the creation of the Joint Chiefs of Staff in 1942 and a federated Department of Defense in 1947.

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foreign and domestic, civilian and military.[9] Further progress may depend on other methods of modifying the attitudes and intellectual equipment of those charged with formulating national security policy at all levels than rearranging the structure of the executive branch.[10]

There has been no comparable effort in adjusting Executive-Congressional relationships to meet the demands of the last fifteen years for a flexible national security policy responsive to changes in the world position of the United States. If anything, governmental arrangements in this area have become even more cumbersome, although the Government Reorganization Act of 1946 reduced the number of committees dealing with military affairs, and committee staffs in general have been strengthened. By assigning specific responsibilities to the President's high-level military subordinates and by permitting top Pentagon military officers to give testimony directly and sometimes in conflict with White House recommendations, Congress has fragmented executive authority.[11]

Prior to World War II, the Congressional power of the purse had, except in actual war, kept our Army small.[12] Probably no amount of executive leadership could have altered this state of affairs. For a European country this might have been a disastrous

[9] Cf. Don K. Price, *Government and Science*, New York, 1954; he comments, in connection with his discussion of the use of scientific advisory boards in defense matters, that "in British constitutional history the right to advise was gradually transformed into the right to make binding decisions" (p. 158).

[10] "The essence of structure is the arrangement of the minds of individual men in terms of the influence that each should have upon the others. The pattern depends upon identifying the types of relevant factors and discriminating among them as to the weight each deserves, the time or times in the movement toward ultimate decision when it may most usefully be injected, and in what degree of detail it should be considered" (Macmahon, *op.cit.*, p. 1). Reorganization then can modify perspectives at top policy levels. Reforms in general education, in pre- and post-entry training, and in career planning may be necessary if it is desired to alter the perspectives of those whose advice and analysis at lower and intermediate levels are channeled up to the ultimate arbiters of policy. Thus, they may further contribute, although indirectly, to an alteration in the range or weighting of considerations at the decision-making level.

[11] The sensitivity of the armed services to Congressional efforts to drive a wedge between the civilian executive and his senior military advisers is shown by the quotations from and comment on the testimony of General J. Lawton Collins in a hearing by the Senate Armed Services Committee on the alleged ammunition shortage in Korea ("Who's Pushing?", *United States Army Combat Forces Journal*, VI, No. 3 [May 1953]).

[12] The Navy was much more generously supported in peacetime than the Army from about 1900 to World War II. For the period after 1933, see Elias Huzar, *The Purse and the Sword*, Ithaca, N.Y., 1950. Small as the defense budget was, its effectiveness was further diminished by legislative restrictions on the spending of money.

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situation, but the United States had the time and space to mobilize after a war crisis had occurred. Once war was declared, however, there were few Congressmen willing to withhold funds needed for mobilization and victory. Thus the problem of executive-legislative relationships was not critical in peacetime and was not important in war.

In this second half of the twentieth century, the security shield of time and space has largely disappeared; more complicated weapons and the necessity for more complete mobilization have lengthened the time it takes to build up arms and armed forces; and armed force is needed quite as much to deter the aggressor as to defeat him. Given these premises, national survival may hinge upon the capacity of the executive and legislative branches to formulate and implement in concert a security program well in advance of any actual war crisis. This program must be kept flexible and is very likely to be expensive. Congress may thus be required to approve huge grants both of funds and of authority.

To make these comments is by no means to suggest that Congress is always wrong and the executive branch (or the Pentagon) always right, and that Congress would serve the country best by simply rubber-stamping executive recommendations. Nor should Congress abandon the watch-dog function which has done much to energize and sanitize the processes of rearmament. There is no monopoly of wisdom in the Pentagon or in the White House and no monopoly of ignorance on the Hill. In fact, the tenure of Cabinet members, undersecretaries and assistant secretaries, and their senior military and diplomatic advisers is shorter than that of the senior members of Congressional committees. The legislator may thus be an informed and formidable antagonist in a committee hearing.

Congress nevertheless works under severe handicaps in its efforts to shape national security policy. Secrecy necessarily shrouds military intelligence and military planning operations. Urgent military problems have suddenly begun to clamor for solution after a long period during which it was perfectly safe for Congress to neglect military affairs. There is the tendency in peacetime, deeply rooted in our Anglo-American political heritage, to regard a strong army as more of a threat to our liberties

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than any foreign foe. Finally, the non-partisan character of many foreign and military policy questions deprives "His Majesty's Loyal Opposition" of much of the incentive to gain expert knowledge of military affairs, for the spur of potential partisan political dividends and irritable domestic pressure groups is lacking. But to list the extenuating circumstances is not to solve the problem of the appropriate executive-legislative relationship for making national security policy.

The test of success in integrating civilian and military considerations is that procedures and means be available so as to identify for the President and for Congress, and more generally for the nation as a whole, the issues and alternative solutions to these issues of national security policy. Properly orchestrated, Professor Macmahon's "concert of advice" and "concert of judgment" should lead to concerted executive-legislative policy. The test is not a procedure so good, or a bureaucracy so wise, that either the President or Congress can safely be figureheads.[13] The continuing external threat to national security permits no "toning-down" of military influence. One may properly ask, however, what are some of the defects of procedure or understanding to be guarded against if national security policy is to be kept responsive to the needs of the country, and if the full range of civilian and military considerations is to be brought to bear on the makers of national policy.

Fleet Admiral Leahy, the first and last "personal Chief of Staff" to Presidents Roosevelt and Truman, has alluded to one of these defects when he describes how frequently frustrated military men sought the Joint Chiefs' assistance in overcoming civilian interference with the execution of military policy. He has also reported that he ordinarily discovered, after careful investigation, "that the civilian agencies were doing an excellent job under difficult conditions." Apparently their actions were based on equally legitimate policy considerations of which the military men had not been and sometimes could not have been aware.[14] Yet with his easy access to the Chief Executive, had he

[13] Cf. Macmahon, *op.cit.*, p. 3: "The potential of drastic choice must exist . . . and be constantly felt in all its parts."

[14] Fleet Admiral William D. Leahy, *I Was There*, New York, 1950, pp. 268-69.

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failed to make a careful investigation, he and the President would have been guilty of what Alfred Vagts calls the vice of immediacy.[15] To make it possible for the official with information and insight on merely a single segment of a problem to have the ear of the Chief Executive on the solution of the whole problem is to commit this vice. It is by no means always the military man who is asked to advise on whole policy on the basis of partial insight. President Wilson before 1916 or 1917 seems to have denied himself the benefit of direct, professional military advice altogether. Germany, as Dr. Vagts points out, may have lost World War I precisely because Admiral Tirpitz was so close to the Kaiser. Diversion of German military energies into naval expansion at Tirpitz' urging may have denied Germany that last measure of land-based power necessary to reach Paris in the summer of 1914 and may have propelled Britain into a still more effective collaboration with France. Certainly the constitutional arrangements in Germany before 1918, and in Japan before 1945, left the determination of force levels largely to the professional military men. The responsible civilian government did not have any orderly opportunity to evaluate the diplomatic consequences of a given military policy.

The wartime shortcircuiting of the civilian Department of State and the civilian leadership of the War and Navy Departments may have made it a little easier for the direction of American military force in World War II to be oriented toward something called "victory" rather than toward a postwar power pattern which would permit the United States to deal with postwar problems. Secretary of War Henry Stimson's dissatisfaction with President Roosevelt's acceptance of recommendations made directly to him by his professional military subordinates perhaps reflected his belief that "the organization which made

[15] This concept is developed by Dr. Vagts in his forthcoming study, "Defense and Diplomacy," an analysis of the soldier's role in the conduct of foreign relations. It is significant that Leahy opposed the creation of a single Chief of Staff to replace the Joint Chiefs, on the ground that his wartime experience had shown him the danger implicit in his own somewhat similar position (Walter Millis, ed., *The Forrestal Diaries*, New York, 1951, pp. 160-61). However, in this context, his concern was to preserve a proper balance not so much between the President's civilian and military advisers as between the President's Army, Navy, and Air Force advisers. This position was in line with the general Navy position in the unification discussion of 1946 (*ibid.*, pp. 159-70).

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the Chiefs of Staff directly responsible to the President . . . might have disturbed him greatly if he had not continued to enjoy a relationship of complete mutual confidence with the President and with Generals Marshall and Arnold."[16] Note that the danger here referred to is not that the military have shown any desire to "take over," but only that levers of power and influence may, when the vice of immediacy persists, fall into the hands of the fractionally insighted, with the result that there may be an unsubtle adjustment of military power to the complex ends of foreign policy.

A second vice is that of fractionalization of national policy. Where each department or service is assigned a mission and goes its own way unless some jurisdictional conflict arises, only disagreements are carried up to the level of the constitutionally responsible chief. Obviously, the President ought to have as clear a grasp of the considerations underlying important decisions on which his subordinates fail to disagree as of those where they do disagree. Furthermore, a gap may persist if each constituent unit defines its private preserve narrowly and presses hardest for a preparedness program in which its distinctive weapons play an enhanced role. If plans for the continental defense of North America and its civilian population and economy failed to keep pace with the Soviet-American atomic arms race, as newspaper reports in 1952 and 1953 of the recommendations of various study groups suggest,[17] this may in part have been due to the way in which missions are assigned in this area.[18] The fractions do not necessarily add up to whole policy.

[16] Henry L. Stimson and McGeorge Bundy, *On Active Service in Peace and War*, New York, 1947, p. 415.

[17] Berkner, *loc.cit.*, p. 260. Cf. Price, *op.cit.*, ch. v, for a discussion of the Department of Defense's use of external advisers, a procedure which may greatly hasten the discovery and the closing of gaps in the assignment of missions. Examples are drawn largely from the field of continental defense problems.

[18] Two "out of Washington" meetings of the Joint Chiefs of Staff and the Secretary of Defense, as well as their respective aides, the Key West and Newport Conferences of 1948, were called to settle interservice differences as to roles and missions. The respective tasks of naval aviation and the Air Force and those of the Marine Corps and the Army were formally delimited in agreements reached at these Conferences. As reported in *The Forrestal Diaries*, pp. 389-96 and 476-77, the conferees were concerned to avoid duplication, while preserving each service's "property rights." There does not seem to have been a comparable effort to make sure that there were no gaps in the assignment of missions.

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In the civilian-military relationship there is one special factor promoting the vices of immediacy and fractionalism. The expanded staff of the White House and of the department heads has greatly strengthened the policy-maker's capacity to evaluate the recommendations of his technical subordinates in non-military fields. There has been no comparable strengthening of civilian staff competent to deal with military problems. It is not so much that the political head of the government needs to check up on his experts' expertness as that he has to balance military and non-military means and make judgments as to what interests are worth securing at what costs. In this he needs the kind of informal help from his White House staff that he already receives in such fields as taxation, agricultural policy, and labor relations. It might not help very much for the Chief Executive (and the Secretary of Defense) to have an independent professional military staff "second-guessing" senior military advisers, when the real need is for aid in balancing and integrating military and non-military considerations. Until there is greater understanding of technical aspects of military problems among civilian administrators, scholars, and journalists, it is difficult to see how the problem can be solved.

In the effort to overcome the defects of immediacy and fractionalism, another vice may manifest itself, the vice of over-coordination. Unless the interagency coordinating groups are carefully restricted, major problem by major problem, "kibitzing" opinions will be brought into play which contaminate expert opinions. The genuinely concerned will introduce considered positions based on systematic and orderly preparation, while the others may operate on a more frivolous basis. It seems to be so easy for an agency concerned with domestic policy to participate in interdepartmental deliberations on questions of defense and diplomacy that over-coordination may almost be said to be a problem peculiar to national security policy. The famous Morgenthau Plan for the transformation of Germany into a pastoral country is only the most spectacular example of an intervention by a department head of a primarily domestic agency into foreign and military policy.

Even in the absence of the over-coordination that leads to

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kibitzing, still a fourth type of organizational defect which results in an unbalanced policy is to have coordination depend upon agreement among parties who have bargaining bases disproportionate to their actual interests or responsibilities. This we may call the vice of the unequal bargaining base. Because only the military has the experience in asking for and spending large sums of money and only the military has the machinery of shipping and supply, its large role in operations may almost automatically have given it a large role in relief and reconstruction policy toward war-devastated areas, whatever the degree of military interest in the operation. Furthermore, when an agency with a large staff negotiates with one with a small staff, the former's abundance of experts, and frequently of high-ranking experts, may weigh in the bargaining balance.

Too much faith in reorganization as a cure for presumed defects in policy-making procedure may lead to tampering with organizational arrangements long before the participants have learned to live with the last previous reorganization. More important, too much faith in reorganization may deflect attention away from deficiencies which are not so simply corrected and from urgent questions of substantive policy. This we might call the vice of over-reorganization. During the great defense unification debate of 1946-1947, changes in national security policy lagged far behind events on the world political scene, possibly because high government officials were preoccupied with reorganization controversies.

Unbalanced or ill-considered policy which flows from inappropriate organizational arrangements can be corrected as rapidly as it is identified by changing the arrangements or learning to live with them. With respect to a second set of obstacles to the development of whole policy, the cure may be more difficult to discover than the diagnosis, for not all attitudes can be modified by administrative reorganization.

One set of attitudes relates to the role of the military or diplomatic expert. If he is so loyal to his political chief that he is always able to discover the technical reasons to support some predetermined policy chosen for political reasons, or if the political chief on his side is insistent that his senior experts give him

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expert reasons for doing what he may be doing for reasons of domestic or foreign politics and testify to those reasons publicly, then the expert is only a kept expert and his so-called expert advice is a fraud perpetrated on the American public. Here the remedy would seem to be to keep the professional military man, the professional diplomat, and the professional industrial mobilizer out of that "fatal crossfire" between the White House and the Hill and simultaneously to build up in our universities, in our journals of opinion, and in our legislatures the expert knowledge it takes to evaluate proposed military policies.[19]

A related and equally objectionable attitude is what we might call the "radar" approach to policy-making. The civilian policy-maker who is willing to support only that level of defense expenditures which the public finds it convenient to pay for and the military expert who recommends that precise level which he estimates the Congress and the White House will stand for are both setting their course to conform to the image which they believe the other fellow wishes to have recorded on his radar screen. In the multi-state system of the modern world, war and diplomacy are played "for keeps" and a demagogic catering to public opinion to reap short-term domestic political dividends is surely the gravest of all threats to a properly concerted national security policy. It leads to that kind of disparity between foreign policy pretensions and military capabilities for which French poli-

[19] General Omar N. Bradley has written concerning his testimony in support of the 1950 defense budget as Chairman of the Joint Chiefs of Staff: "In relation to that budget of $13,000,000,000, as I now look back on it, I can see that I made one serious mistake, even though I acted in good faith. In my testimony before congressional committees I stated: 'We (the Joint Chiefs of Staff) realize that there are certain parts of our defense that are more vulnerable than others. This fiscal year '51 budget does not pretend to fill all the holes. But we hope that this budget *and succeeding ones* will give sufficient emphasis to these points of vulnerability so that the effectiveness of our forces is maximum and the risk to our security is minimum within a few years. If we did not take this view and make this assumption, the appropriation request which we would have to recommend would be out of all proportion to that which we believe this country could afford at this time.' It was that last sentence which I now regard as outside the responsibility of the nation's military advisers. We have no way of knowing what this country *will* afford or its economy *can* afford. Only the economic advisers and the civilian advisers, including Congress, can make the estimate and that decision, and certainly our military recommendations on forces we need should not be curbed in any way by economic assumptions. The President and his economic advisers and the Congress must be presumed to have better knowledge on this subject than the Chiefs of Staff."—"A Soldier's Farewell," *Saturday Evening Post*, CCXXVI, No. 8 (August 22, 1953), pp. 63-64.

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ticians were so freely criticized in connection with the Indo-Chinese War.

Still another attitude which may endanger national security is the slothful attitude of letting crises make policies instead of using policies to avert or meet crises. This attitude we may call the "gyroscope" attitude of spinning along the same old policy line until a Czech coup or Korean War belatedly jolts the country and the government onto a course of action which the expert should long before have seen as obvious and his civilian superior have understood and advocated. We will never know how many lives or dollars we could have saved if the feast-or-famine cycle of military expenditure could have been smoothed out by longer-range military policy planning; another time it may be the life of the nation itself which is unnecessarily sacrificed.

Giving domestic policy considerations an absolute priority over those of external policy may also put the nation's survival in the balance. When Aneurin Bevan in England and Herbert Hoover in the United States attacked existing defense programs on the ground that they would jeopardize the internal economic order, they were implicitly saying that the threat of rearmament to the nation's way of life was greater than the threat of disarmament to the nation's security. Within broad limits, the military resources available to support national security policy are measured by willingness to sacrifice, and this is a power factor which education and leadership can increase.

A guild attitude can also warp policy. If almost all the debate among the professional military men over national security policy is with each other and if the resulting policy recommendation fails to reflect the range of differences of opinion and estimates of the calculated gains and losses from each policy alternative, public opinion will be so starved that it can be moved only by dramatic pronouncements of a "Do this—or else" character. Minimum security needs would then have to be met by leading the public to believe that the nation is forever teetering on the brink of disaster and can only be led to safety by implicit obedience to and faith in the expert's advice.

The tendency to regard national defense as patriotic, in contrast to ordinary politics, which is regarded as dirty, discourages

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any critical examination on their technical and policy merits of professional military recommendations and any constructive harnessing of two equally indispensable groups. The stereotypes of each other which civilians and soldiers may have can also be a powerful barrier to an integration of the efforts of the two groups. The soldier is bureaucratic, overcertain, fatalistic, unimaginative, conservative, and naïve, according to some politicians and diplomats. The soldier on his part has frequently seen the diplomat and the politician as sly, undisciplined, irresolute, irresponsible, indecisive, and sometimes not quite honorable. In a society in which all skills are utilized to support total diplomacy and cold war, it obviously makes little sense for soldiers to be regarded or to regard themselves as a thing apart or for civilians to denigrate soldierly virtues—and the educational system ought to be adjusted accordingly.

Finally, one may list the contrasting predispositional sets of the professional diplomat, soldier, and industrial mobilizer as obstacles to balanced policy development. The mobilizer's concern with "production" and lack of concern with "policy" may or may not be quite all right in actual total war when the armed services can use almost unlimited quantities of everything. When the effort in protracted cold war is as much to deter as to prevent defeat, then production has to be finely geared to complex goals. Neither of the twin objectives, to develop retaliatory and recuperative capacities in actual war and to minimize the chance of the big war occurring at all, must be sacrificed to the other. The military man is quick to acknowledge that aims must be shaped to support policy, but he finds it difficult to understand why policy cannot be set clearly and stay set. He wants to have his military program planned over a period on that basis, with first priority being given to the most serious contingency, a third world war, and to requirements for victory in that war.

The diplomat, on his part, wants to keep the future open, to see military policy shaped so that among the variety of contingencies which may be foreseen, our peacetime military program encourages the occurrence of the contingency most desired, viz., as little a war as possible as little of the time as possible. He is probably not sure, for example, whether a Soviet peace

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offensive of the early post-Stalin period is (1) a grand tactic in the familiar Soviet zigzag; (2) a prelude to a sneak attack; (3) a reaction to the strengthening of NATO forces in Europe and the frustration of Soviet designs in Korea; or (4) a fundamental policy reorientation. To ask the soldier to plan for all these contingencies simultaneously is to ask a good deal. Because the diplomat is trained to be cautious, because he is usually not overly sure of his facts, he wants to keep his policy recommendations tentative in any case. This reinforces the tentativeness which is the result of his desire to preserve future freedom of action.

"Production," "victory," and "peace" as central symbols of policy may not always seem to point in the same direction. Because public opinion is likely to support the expert who makes simple recommendations and makes them in unqualified terms, the diplomat may be unable to hold up his end of a policy debate once it becomes a matter of public controversy. Here again there seems to be no complete remedy through reorganization, but only through more sophistication in international and military affairs both in and out of the government, and especially among those both in and out of uniform whose careers will carry them to policy-making levels.

In simply listing obstacles to the integration of civilian and military considerations in national policy one has not perhaps gone very far in suggesting solution, but a few of the themes which run through this analysis may now be brought together.

(1) For understandable but no longer valid reasons, American political scientists have been slow to investigate the processes by which government adjusts its coercive power to the requirements of foreign and domestic policy. Even now, scholarship and journalism by no means play the role in national security policy that they do in other areas of national policy.

(2) More progress has been made in adjusting the executive branch of the government than the legislative branch (including the President's relations with Congress) to meeting the requirements of coordinated national policy and protracted mobilization.

(3) Reorganization alone will not produce wise national policy. The perspectives of our national policy-makers, both civilian and

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military, must be broadened to take account of each others' special concerns, responsibilities, and competence.

(4) Establishing a proper balance of civilian and military influence is not a matter of reducing military participation in high-level decisions, but of building up civilian competence to weigh in the same balance military and non-military considerations. The expert advice of the military is ever more urgently needed, but final choices under our democratic system are inescapably made by our chosen political leaders, whether or not they are competent to make them.

The cost of failure to integrate civilian and military considerations can be great. It might not be great during the course of a third world war, for a crisis of survival submerges differences and liberates energies. But it is sure to be great in terms of loss of efficiency in the common civilian-military effort to make the peacetime preparations which will minimize that war's cost in lives and treasure and perhaps avoid it altogether. To the extent that civilian and military policy-makers fail to learn to work together, they leave the country a loose and restive coalition which permits the enemy flexibility of maneuver and allows him finally to force war upon us at times and places and under circumstances of his own choosing.

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STATE AND SOCIETY IN NINETEENTH-CENTURY CHINA*

By FRANZ MICHAEL

COMMUNIST China has broken with the Chinese cultural tradition, which it attacks and condemns. This break was prepared in part by the transformation of China during the declining years of imperial rule and in Republican times. But whereas this period was marked by the disintegration and disappearance of old institutions and some uncertainty about things to come, a new, rigid doctrine and social structure are now being introduced to integrate a new society within a totalitarian state. The old values have been discarded, but some of the organizational patterns of the past have been carried over, or have reappeared in the new system. The Communists are attempting to impose their system on Chinese society through the agency of an ideologically oriented elite which not only holds official position in government but also controls society itself. The degree of success which this system achieves may depend in part on the extent to which Chinese society has been prepared by its tradition to accept a centralized bureaucratic state working through a trained elite. In addition to helping us to assess the degree of preconditioning in China for Communist rule, an analytical study of imperial China may provide us with a greater understanding of the social and political techniques which a bureaucratic state employs, and which become of such special importance for a totalitarian government. What, then, were the key features of the imperial state and society which the Communists have retained or replaced in their own way, and what was the role played by the educated elite of the past?

The imperial state aimed at a strong control over Chinese society. The struggle to keep an all-powerful central rule was

* This article is based on a paper read by the author at the Far Eastern Association meeting in New York in April 1954. The data are derived from a number of studies undertaken by several colleagues and the author himself in a cooperative research project on Modern Chinese History at the Far Eastern and Russian Institute at the University of Washington, Seattle.

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the dominant concern of every Chinese dynasty. The center of all authority was the emperor and the court, the embodiment of the interests of the state. Serving the emperor was a group of officials, small in number compared with the size of the country and the population, and with the importance of the functions to be carried out. These officials represented the interests of the state as a whole—its concern with the well-being or acquiescence of all groups of the population. The last Chinese dynasty had in addition special support from a group which served the state without being a part of Chinese society. The Manchus had come as conquerors from the frontier of the Chinese empire, with their forces militarily organized into units known as "banners." When the Manchu dynasty was set up, the Manchu banners were kept apart from the Chinese people and the bannermen remained an inner core of dynastic supporters used both as a military force and in key official positions. But these bannermen were only a small group, largely unqualified for the complex tasks of Chinese administration, and thus the Manchu dynasty, like its predecessors, had to recruit its state administration from Chinese society. To fill the positions in the state administration, the emperor had to be able to draw on a social stratum that was willing and equipped to fill the ranks of bureaucracy and to carry out public functions. This social stratum was the scholar-gentry, a group of educated men whose training gave them a monopoly claim on the holding of office within the imperial bureaucracy.

The bureaucratic aspect of the Chinese imperial state has been recognized, although its importance has sometimes been underestimated. What has not been recognized, however, is the role played by the members of the scholar-gentry, beyond their part as state officials, in the carrying-out of public functions in society. The Manchu dynasty, like its predecessors, set up the comparatively small number of not more than 40,000 official positions for the management of the most urgent tasks in central and local government. At the lowest level of the official hierarchy was the district magistrate, who with his limited personal staff had to administer an area with an average population of 200,000. Many of the public functions in the districts and even in the provinces could not possibly be handled by these officials, or by them alone.

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Hence, to the much larger group of the scholar-gentry fell the task of managing most of the public functions in the administrative areas where they resided. Not only the imperial state but the society itself was bureaucratic—that is, managed by institutionalized public functionaries set apart from the mass of the population. Like the members of the modern totalitarian parties, the members of the scholar-gentry formed a link between society and state, fulfilling their functions in both.

But here the similarity ends. The Chinese imperial state was not totalitarian. Although in theory the emperor was omnipotent, he had no control over the Chinese ideological and social system, even though he tried to influence it. Imperial China did not know the terror of totalitarianism with its organized penetration into all spheres of life. For this reason, the problem of central control over the official organization and, most of all, over the scholar-gentry was never completely solved and remained a constant concern of the dynastic government. The scholar-gentry's interest in the process of state administration could be superseded by their concern to maintain the framework of society and to protect the interests of their group and of their locality, once the state administration became disorderly and lost control. This potential conflict of loyalties and interests, leading again and again to the downfall of dynastic governments, created a continued tension which constituted the dynamics of Chinese bureaucratic development.

Our analysis of the Chinese state and society derived from the last century of imperial rule will thus be focused on this decisive social group, the scholar-gentry. There was a deep social and legal gulf between the small group of so-called *shen-shih*, the educated, the literati, or, as we call them, the gentry, and the large mass of commoners—the poor peasants in the thousands of villages, and the more well-to-do artisans and merchants in the towns. The commoners looked to the gentry for leadership, and whether in appreciation of the merits of their many services, or in fear and bitterness because of their oppression, they respected them as men of learning, as their superiors.

During the last centuries of imperial China, the gentry was a clearly recognizable group. Its members were holders of academic

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degrees. These degrees, gained in official examinations, or in some cases acquired through purchase, gave the holders their privileged position in society. And only on the basis of such an academic degree could anyone receive a government position. The gentry were thus a certified educated group.

Around 1,100,000 persons at the beginning of the nineteenth century and 1,500,000 by the end of the century belonged to this privileged group.[1] The gentry, together with their families, who shared their privileged position, comprised about 5,500,000 to 7,500,000 people, or roughly from 1½ to 2 per cent of the population. Only a very small minority of the gentry could fill the 40,000 official positions which the dynastic government had established. This meant that even with a comparatively high turnover, the large majority of the gentry never came to hold public office. Important as it was, the monopoly on public office was therefore only one of the major aspects of the gentry's role in China. Those who never held office or who were temporarily out of office were, however, by no means excluded from public affairs. On their own, or in cooperation with the officials, they dealt with the many interests of their communities for which official government had no time.

Manifold were the functions thus carried on by the gentry in settling issues through local arbitration, in sponsoring and organizing local charity and welfare matters, grain storage, rice kitchens, and the like, and, most of all, in public works: the vital field of flood prevention, irrigation, and canalization. The building of dams, dikes, canals, roads, and bridges was planned, organized, and supervised by the gentry. In these functions and especially in the field of public works, gentry activity often went beyond the boundaries of a single district and concerned joint works for larger areas in the province. And it must be emphasized that these public functions of the gentry were carried out not within the confines of privately held property, but within the political borders of the district and province to which the gentry belonged. Often such activity required the approval of officials. Sometimes it

[1] The following data and description of the gentry's role is derived from Chang Chung-li, *The Chinese Gentry: Studies on Their Role in Nineteenth-century Chinese Society*, Seattle, Wash., 1954.

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led to joint official-gentry direction and financing, or an officially initiated program was carried out under the guidance and control of local gentry. Indeed, there probably were few public works programs in which the gentry had not some share of responsibility. In this vital field of public affairs and economic control, the gentry thus played a very important part. In times of crisis, as will be shown, this part grew in importance and was extended to military organization and activities as well, which in turn required financial control of regular tax sources.

If we turn from the sphere of practical affairs to the still more important areas of culture and moral beliefs, the gentry's central role is manifest. Confucianism was accepted by the state as the official ideology, but it was studied, taught, believed in, and preached by the gentry, who gave it reality as the focal force of Chinese culture. The gentry were educated and examined in the tenets of Confucianism; and they were understood to have mastered its moral principles as a result of their education. This is not the place to describe the value system of Confucian beliefs, with its emphasis on the dignity of human relations and the importance of social responsibility, represented and interpreted in so many different forms throughout imperial history. In the nineteenth century, Confucianism was still a strong moral force. Then, as before, the gentry were regarded as the guardians of the Confucian tradition. In the eyes of Chinese society, they were the educated elite of morally superior men, qualified to lead society. They preserved and carried on the Chinese cultural tradition. They were the writers, the philosophers, the painters, the poets, and the men of affairs who together made up the greatness of Chinese civilization.

Apart from the literary tradition carried on by the gentry, their cultural activities had a practical side. Teaching was for many a profession. The upkeep of temples or memorial arches, participation in local sacrifices and in the so-called village lectures (*hsiang yüeh*), in which themes from the doctrine were extolled, were important gentry functions. And in their daily life and practices, as well as in the higher field of scholarship, the gentry gave Chinese society its ideological character.

These social functions of the gentry cannot be sharply distin-

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guished from the official functions of the government. They were often intertwined. Whether in or out of office, whether in the interest of the state or of the home community or area, it was one and the same social group that managed public affairs. But the large majority of the gentry were without official position and carried out their activities as social functions, without which, it is true, the government could not have operated. The gentry's dominant position was therefore based not on its monopoly of office-holding alone, but on the whole part it played in Chinese society.

This part was indicated also by the gentry's recognized status and privileged position. The gentry had a preferred position before the law, with a special procedure for the prosecution of gentry crimes and exemptions from humiliating punishment; they had privileges giving them social prestige, such as appearance in public with runners or outriders, with special gowns and hats and buttons; most of all, they had economic privileges. In recognition of their gentry services, they were free from the *corvée*, and they could use their position to escape the onerous surtaxes and lighten their general tax burden. Altogether, the gentry were apart from and above the mass of the commoners, on a par with the officials before whom the commoners had to prostrate themselves; the officials, themselves members of the gentry, were equals with whom social contact made easy the promotion of common interests.

This elevated position of the gentry in Chinese society was therefore based on their accepted role as managers of public affairs. The gentry themselves believed in such a responsibility. The statement of their obligation by Fan Chung-yen in Sung times: "Hsien t'ien-hsia chih yu erh yu; hou t'ien-hsia chih lo erh lo," "To be first in worrying about the world's troubles and last in the enjoyment of its pleasures," indicates their feeling of dedication to selfless service which, if not always lived up to, was believed in as a standard of gentry behavior. And in the mid-nineteenth century the famous scholar, Tseng Kuo-fan, called the gentry to battle in defense of their society. When the Taipings threatened Confucianism with their new ideology, Tseng exclaimed:

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> How could that be a change that only concerns our Ch'ing dynasty? No, it is a serious change that concerns our entire civilization (*ming chiao*) from its very beginnings, and makes our Confucius and our Mencius cry with grief at the nine wells. How can anyone who can read and write remain quietly seated, hands in sleeves, without thinking of doing something about it?

To arouse the gentry to organize their communities and to lead them in the fight, he used again and again the famous quotation from Ku Yen-wu at the end of the Ming dynasty: "T'ien-hsia hsing wang, p'i-fu yu tse," "The fortune or misfortune of the world rests on the shoulders of each of us." Leadership of the community, more urgent in times of emergency, was always an accepted task for the gentry. The gentry's knowledge of the principles of human affairs and human relations and of the ethical tenets which were believed to rule them qualified the gentry to organize and lead the people in these public tasks. The basis of the gentry's power was knowledge and the acceptance of their intellectual status by society.

This definition runs counter to a frequently held opinion that the position and power of the Chinese gentry were based on landownership. The ownership of land, from which the gentry are believed by some to have derived their main income, is held to determine, in an agricultural society such as China, the gentry's local power and with it their dominant role in society and state. What was the gentry's position in Chinese economy? Their public functions included, as we have seen, the management and control of public works as well as official administrative control of the economy, and this was reflected in their income. A study of the nineteenth-century Chinese national product and of the gentry's share in it, based on figures of the 1880's, has provided us with economic data that parallel our findings on the importance of the gentry's public role in managing Chinese society and state.[2] As the dominant group in society, monopolizing government office, the gentry could be expected to have had a share of the national income larger than the group's proportional size in the total population. Our figures indicate that the gentry—making up

[2] For the following data, see Chang Chung-li, *The Gentry in Nineteenth-century China: Their Economic Position as Evidenced by Their Share of the National Product*, doctoral dissertation, University of Washington, 1953.

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about 2 per cent of the population—received about 23 per cent of the national income. Of greater importance, however, is a breakdown of this gentry income according to the sources from which it was derived. Much of the irregular income from official positions or public functions—the bribes, the squeeze, and all the other extras that were improper and therefore secret—is not easy to ascertain or to determine in figures. It is therefore calculated very conservatively in this study, and it is possible that this sort of income was considerably higher. The regular income is more easily evaluated. On the basis of this regular income and a conservative estimate of the irregular income, the total gentry income is estimated as being derived almost 50 per cent from services, including official salaries, compensations for gentry service of many kinds, such activities as teaching, and so forth; a little less than a third came from landownership; and about one-fifth from entrepreneurial or business activities of one kind or another, the latter figure increasing slightly toward the end of the nineteenth century. This, it must be understood, was the proportionate income for the gentry as a whole.

These figures become even more remarkable when divided among the various groups within the gentry itself. We believe that a distinction can be made between the upper gentry, consisting of the holders of the higher examination degrees and official ranks, on the one hand, and the rank and file of the lower degree-holders, whether they were possessors of examination or purchased degrees, on the other. The former group, the upper gentry, consisted in our estimate of about 14 per cent of the total gentry, leaving the large majority of 86 per cent as lower gentry. One would expect to find the income from rents going largely to the lower gentry, which excluded all officeholders and the most influential gentry figures. But the data indicate that most of the rent income, about three-fourths of it, went to the upper gentry. In fact, it went to a very small proportion of them. Less than 10 per cent of the upper gentry had the largest share of the total rent income of the gentry. These figures show us that the large majority of the gentry, especially the lower gentry, but also most of the upper gentry, received their main income not from land but from public services. The vast majority of the gentry thus

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received their main income from their paramount public functions. And those upper gentry families who were large landowners, and of whom we know (for instance, Tseng Kuo-fan's family), usually acquired their land after they gained prominence and wealth in public service. With the division of property through inheritance laws and the uncertainty of positions of political influence, the acquisition of landed property was not the basis of political power.

We believe these findings support our view of the character of Chinese society, a society in which an educated elite, as officials of the state and as the leading social group, managed vital public functions—including the all-important public works—and derived its dominant position from this monopoly of management.

Any state organization that depends, as did the Chinese state, on the services of such a monopoly group faces a tremendous problem of integration and of central control. The state had to curb and restrain the individual, clique, and group interests of the members of the gentry, and to merge them with the common interests of the whole community. This interest of the Chinese state was most obviously expressed by the emperor and the dynastic family itself. To get control of the gentry, to get them into his "basket," to have them depend on his central authority rather than on an autonomous authority of their own, was a main aim of the emperor's government. And the imperial Chinese state had developed an elaborate, intricate, and highly sophisticated system to gain and maintain such central control.

The control of the gentry was of course only one part of the broader problem of dynastic authority in a society the framework of which the emperors could use but could not determine. Within that society, other organizational rivals challenged at times the power of the official government. From the time of the Sung dynasty at least, secret societies and their branches had formed illegal organizations within the state and created periodic open rebellions against the authority of the existing government. These groups had a different type of social leadership and often in their original form maintained a more equalitarian ideology derived from Buddhist or Taoist beliefs. But for any larger positive effort to create administrations of their own, secret

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societies or other such groups depended on the services of qualified educated men. In any political conflict, there was thus always the struggle to maintain a central authority over the obedience and services of the educated elite, and to limit and curb the possibility of its forming or supporting rival organizations.

The measures of control used under the Ch'ing dynasty were in the main not new, but were in several ways more refined than those of its dynastic predecessors. The examination system itself was an important measure of control. Through it, the state determined who became a member of the gentry and through what qualifications. The quotas which were set for the examinations limited the size of the gentry and kept it manageable. Regardless of the vast number of local applicants, the quotas provided only a limited number of gentry members for each district to carry out the gentry functions. The government's control over the content of the examinations permitted it to stress the official aspects of Confucian philosophy or the importance of form. Review examinations, control of the gentry through the local schools, and stipends supervised by state-appointed educational commissioners were other checks. Indeed, the constantly induced preoccupation of the gentry with preparation for examinations—"the examination life"—was a method of channeling the gentry's energies that permitted little time for undesirable developments in thought or action.

These measures, which can only be touched upon here, dealt with the gentry itself. Others dealt with the communities.[3] Main examples were the *pao-chia* and *li-chia* systems, which, according to the original plan, handled local police control and tax collection, respectively. By dividing the communities arbitrarily into such subadministrative units and placing commoners in the position of local agents of the state, responsible to the officials for security or tax payment, the government undermined the autonomous position of the communities and handicapped their leadership. Although such measures did not function as planned for any great length of time, they had a stimying effect on the communities, providing at least a temporary safeguard against

[3] See Hsiao Kung-chuan, *Rural China: Imperial Control in the Nineteenth Century*, in manuscript.

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any form of local autonomy. While the state used the gentry for local control, it thus introduced at the same time its own direct local organizations.

More sophisticated still were the measures, in force up until the Taiping Rebellion, to maintain central control within the group of gentry serving the state as officials. No official was to serve in his home district or province, so that there would be no chance for the growth of local roots of power. For the same reason, officials held office for only short periods of time and were frequently shifted from place to place. Another precaution taken at the expense of effective administration was both the division and the overlapping of the functions of officials on all top and intermediate levels. An intricate system of divided authorities, of mutual checks and secret reports, was designed to keep all threads in the hands of the emperor. To manage it, however, he needed an inner core of officials of special loyalty. Studies on official careers indicate that there was an inner group of central officials into which those who had once gained important positions in the provinces were not again admitted.[4] This central group was then expected to represent, like the emperor himself, the interests of the central government with which it was so closely linked.

In addition, the Manchu dynasty used its own people as a check on Chinese society. The Manchus, artificially kept apart from the Chinese in their banner organizations, were used as garrisons in strategic locations as a military safeguard for the dynasty. Moreover, they held jointly with Chinese the key offices of the central government and filled some of the key positions in the provinces.

This whole elaborate mechanism for maintaining central authority succeeded only temporarily. Toward the end of each dynastic period, the very problems which it tried to exclude recurred. In the nineteenth century, the Ch'ing government was in decline. The growing economic strength and independence of the local gentry, the growing corruption and demoralization of the administration, paralyzed by its own system of controls,

[4] See, for instance, Alfred Kuo-liang Ho, "The Grand Council in the Ch'ing Dynasty," *Far Eastern Quarterly*, IX, No. 2 (February 1952), pp. 167-82.

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brought a weakening of the central authority and a strengthening of local and eventually regional autonomy, in which both local officials and scholar-gentry became increasingly independent of the central government. The less effective the government became, the more opportunities there were for local mismanagement and exploitation, and the more local unrest, banditry, and uprisings resulted. The impotence of the official administration gave local leaders, secret society heads, or local bullies the opportunity to expand their influence. And the gentry's social functions, which had fitted so well into the state system, took on a different character when the administration and its central control crumbled. First things came first, and the gentry, too, organized communities for defense in the spreading local struggle. Quarrels between local communities or clans, resistance against exploitation, defense against the growing danger of banditry, itself caused by economic misery, led to the development of local military organizations. The regular government troops became corrupt and ineffective, together with the rest of the administration, and local forces took matters into their own hands. The very danger appeared which the government had tried to avoid, the emergence of local leaders with their own resources of power. The gentry or secret society leaders who formed local corps commanded forces personally loyal to them. They controlled areas from which they derived their financial support, and they built up staff organizations of men appointed by them and loyal to their group. As soon as these organizations grew beyond the level of local intercommunity rivalry and affected a larger region, they became an open political challenge to the dynastic regime.

The Taiping Rebellion, in the middle of the nineteenth century, thus grew from the level of local organization and local conflicts into a rebellious movement, through the appeal of a fanatical ideology taken in part from Christianity, with precepts that seemed to promise correction of some of the political, social, and economic ills of the time. The dynastic government was truly incapable of defense. But the local gentry, who could not cooperate with a movement so alien to the cultural tradition which they represented, set up their own defenses based on their own local sources of power. When the official leadership through the

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state failed, the gentry expanded their activities. They formed military organizations, and to finance them they at first levied contributions but soon established their own tax organizations, which were practically uncontrolled by the state. From local beginnings grew regional machines, with military, financial, and administrative power.

The story of the build-up of regional armies out of gentry-led local corps by Tseng Kuo-fan and others like him cannot be told here. After trying to keep down these regional defenders of the Chinese cultural tradition, the dynastic government, following the defeat of its own armies, finally had no choice but to grant to these men the actual administrative offices of the provinces. These new regional leaders and their military organizations were far more successful in building up administrative and military control over the areas within their reach than were the Taipings themselves. Area by area, and city by city, they destroyed the Taiping forces until they took the Taiping capital, Nanking, and crushed the rebellion.

But while fighting for the government of the Ch'ing dynasty, these gentry leaders established their own power. Even after the crisis was over, some of them retained their military forces, their financial resources, and the administrative organizations which they had created in the areas occupied by them. The dynastic government had no choice but to continue to appoint these men to positions as governors and governors-general in these areas, and thus to sanction the growth of regional power. Even if individual leaders were removed, the organizations remained, and while the government retained the authority to appoint officials, to dismiss them, or to shift them from place to place, it could not destroy the organizational roots of their power or the personal loyalties of officers and officials who had been the protégés of such leaders. When the head of such an organization was removed to another province, he took with him some of his troops, offered appointments to his friends, and frequently established new sources of income without giving up his old ones. The elaborate system of separation of gentry from officials, of mutual checks on the officials, and of division of functions and authority broke down. There developed a new type of provincial governor-general, who

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had military, financial, and administrative power of his own and had to be consulted by the throne on major policy matters. With the growing importance of foreign relations, some of these men even began to deal directly with foreign powers. Chinese foreign policy, then, became linked with the inner struggle.

Why did this system of regional developments not lead to a new reintegration of power? To attempt an answer would carry us far beyond the scope of this article. But it may be pointed out that the leading group in society and state, the scholar-gentry, lost its functional as well as its intellectual and spiritual basis at the turn of the century, partly as the result of the influence of the West.

The leaders of the first regional army formed against the Taiping Rebellion were, like their commander Tseng Kuo-fan, Confucian scholars who aimed at maintaining the traditional gentry ideals. When the movement of regional organization spread from the rural interior to the coast and the coastal cities, the traditional social basis and, with it, its moral force were lost. The regional organization of Li Hung-chang exploited the new commercial and industrial opportunities on the coast and applied to them the methods of gentry-official control formerly exercised in agricultural China. A last attempt to revive the Confucian state on the basis of a reformed Confucian belief was made by the philosopher K'ang Yu-wei, but it failed. And with the end of the traditional educational system, marked by the abolition of examinations, the sustaining moral force of the dominant scholar-gentry class disappeared, and so did the traditional scholar-gentry itself. There survived into Republican times a residue of bureaucratic tradition which has reappeared in the new Communist bureaucratic state and society, where a new indoctrinated elite plays a leading part, but without any of the ethical values once held high by the Confucian scholars.

From this analysis, many parallels suggest themselves between the institutions of the imperial state and society and the role of the gentry, on the one hand, and the institutions of the Communist state and the Communist Party, on the other. The general management of affairs by an elite group; the relationship of functions handled by members of the elite through their official

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positions to those handled through their role in society; the role of the elite as carrier and propagator of the dominant ideology; the restricted size of the elite group and the control of admission to it; the channeling of thought—in all these and in many other characteristics, the imperial structure shows strong similarities to the Communist system. What is different today is that the leadership of the elite party is at the same time at the head of the state government. What is also different is the Communist combination of a central institutional control with a control over the ideology itself. No independent value system remains, and the ideological independence of the gentry which made possible the inner balance of the imperial system has been replaced by an ideological centralism in which right and wrong are always determined by the total authority of those on the Communist throne.

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CONDITIONS AND PROSPECTS FOR ECONOMIC GROWTH IN COMMUNIST CHINA*

By ALEXANDER ECKSTEIN

IV. Prospects for Economic Growth: A Model

Regardless of whether a detailed Five-Year Plan is in operation or not, there is in Communist China every indication of a determined, relentless, and massive effort to pursue a program of industrial expansion. The rapid rate of recovery, the restructuring of the institutional framework, the possession of an industrial base in Manchuria, the termination of hostilities in Korea, and, above all, the application of political and social power to the mobilization of resources in the hands of the state are all factors that have enabled the regime to raise the rate and level of investment considerably above that of the past. At the same time, the regime is mobilizing not only capital, but technique and entrepreneurship as well. In essence, the Chinese economy—after being more or less stationary for centuries, with only erratic and partial spurts of growth in recent decades—seems to be entering, for better or for worse, a self-sustaining growth process.

Barring another world war or a major agricultural crisis in China, the long-run question before us is not whether the Chinese Communist economy will grow at all, but whether the rate of growth will be sufficiently rapid so that the forces of the industrial revolution will be in a position to defeat the Malthusian counter-revolution. Given the previously discussed conditions and limitations, how rapidly may industrialization be expected to proceed? Obviously, these questions can only be answered conditionally and hypothetically. However, before approaching this problem, it may be well to take a schematic look at the "size" and "structure" of the Chinese mainland economy in 1952, which may be considered as the point of departure for the upward climb.

* This study is concluded with Part IV. Parts I and II appeared in the October 1954 issue of *World Politics*, Part III in January 1955.

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The data in Table 10 are based on some quite tentative and crude calculations and are, therefore, intended merely as indications of approximate orders of magnitude rather than as actual estimates.

TABLE 10

GROSS NATIONAL PRODUCT AND ITS COMPOSITION IN 1952

Product		*Expenditure*	
By Sector	(in per cent)	*By Use*	(in per cent)
Agriculture	40	Consumption of households	73
Small-scale and rural industry	15	Communal services	4
Factory industry and mining	7	Government administration	4
Trade and transport	24	Military outlays	7
Dwelling services	4	Investment	12
Government and other services	10		100
	100		
Total, in billions of US dollars	30.0		30.0

In many respects, this is a pattern that is fairly typical for many underdeveloped areas. However, it exhibits several unique characteristics, the most outstanding of which is the large share mobilized by the government and appropriated for its own use. This is, of course, but another indication of the degree to which the Chinese economy had been nationalized by 1952. As a result, this share is higher than in any other country in Asia. Even more important in this context is the high rate of investment as compared with that in other underdeveloped countries. For example, the rate of investment in India was estimated at about 8 per cent in 1949-1950.[1]

The investment share given above is based on government budget figures in Table 8. Thus, it leaves private investment out of account. By 1952, such investment was probably negligible in the urban sector; but in the rural sector there was undoubtedly some non-monetary investment. Owing to the very character of

[1] This relates the share of gross investment to gross national product. Derived from M. Mukharjee and A. U. Ghosh, "The Pattern of Income and Expenditure in the Indian Union," *Bulletin of the International Institute of Statistics*, XXXIII, No. 3 (December 1951), pp. 49-68.

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government budgeting and accounting, an extremely broad concept of investment is used in Communist China. As a matter of fact, the term officially applied is expenditures on "economic construction"; these include outlays both on fixed and on working capital, including, for instance, raw material and fertilizer credits.

According to aforementioned budget figures, investment resources were allocated as shown in Table 11.

TABLE 11

ALLOCATION OF INVESTMENT RESOURCES, 1953-1954
(in per cent)

	1953		1954
	Budget	*Actual*	*Budget*
Industry, including mining, manufacturing, and electric power	46.0	49.9	47.8
Agriculture, forestry, and water conservation	11.4	13.2	10.5
Railway and communications	14.3	14.5	15.5
Trade and banking	4.3	11.5	11.3
Non-specified projects	24.0	10.9	14.9

Assuming that the Chinese follow Soviet budget practice, one may legitimately surmise that the non-specified projects are partly military in character; however, they may also include investments in government overhead, social and cultural services, and a wide miscellany of small projects. On a more general plane, it is interesting to note the striking similarities in the Soviet and Chinese patterns of investment allocation, with the marked emphasis upon, and high priority accorded to, producers' goods industries in both cases. Thus, while 45 to 50 per cent of total investment was allocated to industry in both countries, the bulk of this—slightly over 80 per cent in the USSR, and somewhat under 80 per cent in China—was devoted to the expansion of the producers' goods branches. At the same time, it seems that agriculture in China is kept on an even shorter investment ration than in the Soviet Union, with only 10 to 13 per cent of the investment allocated to the former as compared with 19 per cent in the latter. This, however, may be largely due to the fact that rapid collectivization, accompanied by a large loss of livestock, forced a

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speedy substitution of machines for animals and labor in Soviet agriculture.[2]

Heretofore our analysis of the dynamics of economic growth in Communist China has been confined to an examination of prospects in terms of specific factors and sectors, such as resource endowments, agricultural policies, planning practices, and investment patterns. On the other hand, the model in Table 12 attempts to combine these interrelated elements into a projection of the potentialities for aggregate and industrial growth in mainland China. Proceeding from the 1952 estimates of population, gross national product and expenditure, and published government budget data on levels and patterns of investment, a hypothetical growth model was constructed as follows:

(1) Increments in national product and industrial output were treated as a function of aggregate and industrial investment, respectively. It was thus assumed that for every 3.5 units of net investment made during one year, national product would increase by one unit during the following year (i.e., that the incremental capital-output ratio would be 3.5:1 with a one-year investment lag). Similarly, it was assumed that for every unit increase in industrial output, four units of investment would be required; and that the currently prevailing investment share for industry—about 45 per cent—would be maintained throughout the decade.

(2) Personal consumption was treated as a function of population growth and urbanization; specifically, it was assumed that (a) population would grow at a rate of 1.25 per annum; (b) urbanization would be a function of the rate of industrial growth, with urban population expanding at one-quarter of the industrial rate; and (c), starting with a 1952 per capita rural-urban consumption ratio of 1:1.8, per capita consumption levels in each sector would remain constant.

(3) Government consumption was projected on the assumption that the share of gross national product devoted to military expenditure and social overhead for administration and for community services would remain constant.

[2] Soviet investment figures cited here refer to the period of the first two Five-Year Plans and are based on Norman Kaplan's study, *Capital Investments in the Soviet Union, 1924-1951*, Rand Research Memorandum 735, Santa Monica, Calif., November 1951.

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(4) Investment was derived as a residual, after deducting personal and government consumption from national product.

The choice of an incremental capital-output ratio is a most complex problem, particularly for a country such as China, where there is no sustained development experience to draw upon. At best, the capital-output ratio is a more or less rough-and-ready average which is a product of a host of counteracting tendencies and policies at work in the economy.[3] For purposes of the model in Table 12, this choice was based on an analysis of the Soviet and Japanese growth experience, on the one hand, and on current economic realities and policies in Communist China, on the other. Moreover, our selection was based on the hypothesis that the capital-output ratio is ultimately determined by the resource-population balance, the pattern of investment, and the level of technology and entrepreneurship available in the economy. In other words, the more favorable the population-resource balance, the more advanced the technology and the entrepreneurial skills, and the lower the relative investment share of social overhead and other sectors in which the capital coefficient is high, the lower will tend to be the aggregate incremental ratio.

On the basis of our current state of knowledge concerning national product and capital formation in the USSR between 1928 and 1937, it would seem that the incremental capital-output ratio for that decade was no higher, and possibly lower, than 3.[4] For Japan, there are studies in progress that definitely indicate that Colin Clark's ratios are too high. While there are as yet no reliable estimates of the ratios as such, recently published national product estimates for 1878-1942 show that the rates of growth for 1880-1900 were close to 5 per cent per annum as compared with Clark's estimates of less than 2 per cent.[5] If one accepts the

[3] Capital-output ratios used here specifically refer to incremental ratios designed to measure the increment in product associated with a given amount of additional (new) net investment.

[4] See Alexander Eckstein and Peter Gutman, "Note on the Soviet Incremental and Average Capital-Output Ratios," CENIS Econ. Devt., A.I.6, Cambridge, Mass., March 18, 1954 (mimeograph).

[5] Shigeto Tsuru and Kazushi Ohkawa, "Long-term Changes in the National Product of Japan Since 1878," *Income and Wealth*, Series III, Cambridge, Eng., 1953; Yuzo Yamada and Associates, *Notes on the Income Growth and the Rate of Saving in Japan*, Mimeo. 19, Tokyo, 1953; Colin Clark, *The Conditions of Economic Progress*, 2nd ed., London, 1951, pp. 136-38.

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5 per cent rate, then it is most doubtful that the incremental capital-output ratio could possibly have been higher than 4, or maybe even 3, since this would require a net rate of investment of from 15 to 20 per cent. A 20 per cent rate of investment would seem extraordinary indeed for an economy at that stage of development.

In terms of the factors discussed above, one could expect the incremental capital-output ratio in Communist China to be possibly lower than in Meiji Japan, but higher than in the Soviet Union. The resource-population balance was undoubtedly more favorable in the USSR of 1928, but less propitious in Meiji Japan. The poor quality of Chinese coking coal and iron ore, coupled with the fact that frequently the distance between deposits of these two key raw materials is quite considerable, would tend to raise the capital-output ratio. This may be counterbalanced to some extent by the fact that there may be greater opportunities for utilizing underemployed rural labor for capital formation in China than in the Soviet Union. So far as the level of technology and entrepreneurship is concerned, Communist China may again assume an intermediate position between the two countries; in this respect, China probably has a considerable advantage over Meiji Japan, since it is starting with a relatively larger industrial base and from a more advanced stage of world technological development. On the other hand, the Russia of 1928 was technically and educationally much more advanced than the China of 1952. The rate of literacy, the number of technicians and skilled workers, and the general state of the arts were all higher.

A comparison of the pattern of investment in the three countries yields a less clear-cut conclusion as to the relative magnitudes of the capital-output ratios. China, with a much more underdeveloped transport system than the USSR, seems to be allocating a smaller share of its total investment resources to this sector than the Soviet Union did during 1928-1937. However, the bulk of this railroad development in China is concentrated in sparsely populated, remote areas where transport returns are bound to be low, and the sectoral capital-output ratios quite high. Owing to its geographic configuration, Japan was less dependent upon

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highly capital-intensive railroad development. On the other side of the balance sheet, the Soviet Union had to pay relatively more attention than China to the expansion of its electric power facilities, in view of its very small generating capacity in 1928. Housing—another sector with a high incremental capital-output ratio—was notoriously neglected in the USSR, and China may be expected to follow suit. It must be noted, however, that opportunities for economizing on housing space through overcrowding may be much more limited in China than they were in Russia. Moreover, while the initial cost of residential construction is undoubtedly lower in China, relative costs of replacement and maintenance may be much higher. However, differences between the patterns of investment allocation in the three countries are not confined to the social overhead sectors. Thus, Meiji Japan concentrated its efforts upon the development of agriculture and of the textile industry, with the latter based to a considerable extent upon small-scale, cottage-type manufacture.[6] In contrast, both the Soviet Union and Communist China seem to accord a considerably lower priority to agricultural development and a much higher priority to the expansion of producers' goods industries. These, of course, would be factors making for a comparatively lower capital-output ratio in Meiji Japan.

On the basis of all of these considerations combined, an aggregate incremental capital-output ratio of 3.5 and an industrial ratio of 4.0 were selected as most reasonable for this first decade of deliberate industrialization in China. The ratios applied in the model were based on net, rather than gross, investment. For this purpose, aggregate and industrial capital stock was estimated for 1952, with net additions to this stock computed on the basis of the following capital consumption allowances:

	1952 Stock	*New Stock*[a]
	(in per cent per year)	
Aggregate	4.0	2.0
Industrial	7.0	3.5

[a] These rates applied only after 1957, on the assumption that even some of the new capital will have to begin to be replaced after a few years.

[6] See W. W. Lockwood, *The Economic Development of Japan*, Princeton, N.J., 1954, pp. 25-34.

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The 1952 aggregate capital stock figure is strictly arbitrary, based on an assumed average capital-output ratio of 1.33. However, the industrial stock figure is based on a prewar estimate for China proper and a 1945 estimate for Manchuria, both converted into 1952 prices.[7]

As was indicated above, the model in Table 12 is based on the assumption that population will grow at an aggregate rate of 1.25 per cent a year. However, owing to rural-urban migration under the impact of industrial and other closely related demands for labor, urban population will grow at a much more rapid rate than rural. An examination of Japanese and Soviet rates of urbanization[8] indicates that, in the first case, roughly a trebling in industrial output was accompanied by a doubling in the urban population; in the second case, a doubling in urban population was paralleled by a quadrupling in industrial production. Given the respective character of Japanese and Soviet economic development, this is not at all surprising. The much greater capital intensity of the Soviet process of economic growth, with its heavy emphasis upon expansion of producers' goods industries, naturally meant that the urban demand for labor per unit of output was lower than in Japan, where textiles and other light industries played a much greater role. On the basis of these considerations, it was assumed that urbanization in China would proceed at a rate that would be closer to the Soviet than the Japanese tempo. This assumption is based on the fact that the Chinese Communist regime seems to place much greater emphasis upon the development of heavy industry than the Japanese did in the first decades following the Meiji restoration. Recent evidence seems to indicate that Chinese Communist planners are allocating roughly the same share of investment resources to the expansion of heavy industry as the Soviet Union did during the period of its first two Five-Year Plans.[9]

[7] Koh Tso-fan, "Capital Stock in China," *Problems of Economic Reconstruction in China*, China Council Paper No. 2, IPR, Eighth Conference at Mont Tremblant, December 1942; Edwin W. Pauley, *Report on Japanese Assets in Manchuria to the President of the United States*, July 1946.

[8] This applies to Japan approximately between 1890 and 1915 and to the Soviet Union between 1928 and 1938.

[9] See Teng Hsiao-ping, "Report on the 1954 State Budget," in *NCNA Daily Bulletin*, Supplement No. 204, London, June 29, 1954.

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TABLE 12
MODEL OF ECONOMIC GROWTH IN COMMUNIST CHINA
(in billions of 1952 US dollars)

Year	Gross Natl. Prod.		National	Consumption				Aggregate Investment		Industrial Investment	
	Dollars	Index	Income	Pers.	Mil.	Other Gov't	Total	Gross	Net	Gross	Net
1952	30.0	100.0	28.4	21.9	2.1	2.4	26.4	3.6	2.0	1.6	1.2
1953	30.5	101.7	29.0	22.2	2.1	2.4	26.8	3.7	2.2	1.7	1.3
1954	31.1	103.7	29.6	22.5	2.2	2.5	27.2	3.9	2.4	1.7	1.4
1955	31.7	105.7	30.3	22.9	2.2	2.5	27.6	4.0	2.6	1.8	1.5
1956	32.4	108.0	31.0	23.2	2.3	2.6	28.1	4.3	2.9	1.9	1.6
1957	33.2	110.7	31.9	23.6	2.3	2.6	28.6	4.6	3.3	2.1	1.8
1958	34.6	115.3	32.8	24.0	2.4	2.7	29.2	5.4	3.7	2.4	1.8
1959	35.7	119.0	33.9	24.3	2.5	2.8	29.6	6.0	4.2	2.7	2.0
1960	36.9	123.0	35.1	24.7	2.6	2.9	30.2	6.7	4.9	3.0	2.2
1961	38.4	128.0	36.5	25.1	2.7	3.1	30.8	7.6	5.7	3.4	2.5
1962	40.2	134.0	38.1	25.4	2.8	3.2	31.4	8.7	6.7	3.9	2.9

The level of aggregate consumption will be in part a function of this urbanization, since available evidence would tend to indicate that standards of consumption are higher in the cities than in the countryside. To this must be added the fact that in a largely pre-industrial society the resource costs of maintaining a person in the city are higher than in the countryside. Therefore, proceeding from Indian data on rural-urban consumption ratios and allowing for differences in transport costs and direction of net resource flows in the two countries, it was assumed that urban consumption in China would be 1.8 times the rural average.

Moreover, it was assumed that government consumption would increase at a more rapid rate than personal consumption, i.e., it would expand at the same rate as gross national product. This assumption is based on the hypothesis that maintenance and expansion of the state apparatus to assure the degree of control desired and required by the regime for resource mobilization and allocation and for non-economic ends will necessarily involve increasing unit costs. Moreover, with the high priorities assigned to modernizing and reequipping the army, unit costs of maintaining a large standing army are also likely to rise.

In appraising the computations in Table 12, it should be noted that this type of model is highly sensitive, so that even a comparatively small change in assumptions may yield vastly different results. Thus, should all variables such as population growth, the rate of urbanization, the rural-urban consumption

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Industrial Output		*Population*			*Personal Consumption*		*Total Capital Stock at Beginning of Year*			*Industrial Capital Stock at Beginning of Year*		
Dollars	Index	Total	Urban	Rural	Urban	Rural	Old Cap. Remaining	New Cap. Remaining	Total	Old Cap. Remaining	New Cap. Remaining	Total
		(in millions)										
2.1	100.0	580	116	464	6.8	15.1	40.0	–	40.0	6.0	–	6.0
2.4	114.3	587	120	467	7.0	15.2	38.4	3.6	42.0	5.6	1.6	7.2
2.7	128.6	594	124	470	7.3	15.3	36.9	7.3	44.2	5.2	3.3	8.5
3.1	147.6	602	128	474	7.5	15.4	35.4	11.2	46.6	4.8	5.0	9.8
3.4	161.9	610	132	478	7.7	15.5	34.0	15.2	49.2	4.5	6.9	11.4
3.8	180.9	617	136	481	7.9	15.6	32.6	19.5	52.1	4.2	8.8	13.0
4.2	200.0	625	140	486	8.2	15.8	31.3	24.1	55.4	3.9	10.9	14.2
4.7	223.8	633	144	489	8.4	15.9	30.0	29.5	59.5	3.6	13.3	16.9
5.2	247.6	641	148	493	8.7	16.0	28.8	35.5	64.3	3.4	16.0	19.4
5.7	271.4	648	151	497	8.9	16.2	27.7	42.2	69.9	3.1	19.0	22.1
6.4	304.8	656	155	501	9.1	16.3	26.6	49.8	76.4	2.9	22.4	25.3

differential, and the incremental capital-output ratio be higher, the rate of growth would of course be greatly reduced. On the other hand, should some or all of these variables be lower, significantly higher rates of growth than those postulated would result. Our analysis is focused on what may be considered as the intermediate model, based on assumptions which, in terms of current Chinese realities, evaluated against the background of Japanese and Soviet experience, seemed to be the most plausible ones. One of the most serious limitations of this model is that the assumptions on which it is based are not changed. As a result, the model tends to become less and less applicable as the projections are carried further into the future.

Bearing these limitations in mind, on the basis of these assumptions the Chinese Communist economy may be expected to grow at a rate of less than 2 per cent a year at the beginning, a rate which under the cumulative impact of the growth process would be stepped up to almost 5 per cent a year by the end of the decade. Thus, toward the latter half of this period, the Chinese economy would be expanding at a rate roughly corresponding to that of Meiji Japan, but lagging appreciably behind the Soviet tempo of 1928-1937.[10] Industrial output in China would, however, be growing so rapidly that it would be trebled during the decade, and its share in gross national product would be doubled. This

[10] Abram Bergson, ed., *Soviet Economic Growth: Conditions and Perspectives*, Evanston, Ill., 1953, p. 9.

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rapid pace of industrial growth would be due to the large additions of new plant and equipment to a very modest industrial base. However, precisely because such a large share of investment is devoted to industry while industry's share in gross national product is small (7 per cent in 1952), the aggregate rate of growth would be relatively slow during the first years, despite the rapid tempo of industrial expansion. This situation changes only in later years, as industry begins to have a greater weight in the economy as a whole.

One of the central elements in this process of growth is a rapidly rising productivity of labor under the impact of complex and interrelated forces. On the basis of all of the data available, it would seem that the more underdeveloped an economy, the greater tend to be the differentials between the labor products of the economic sectors. Under such conditions, the very shift of labor out of agriculture—a low productivity sector—into urban, factory industry—a high productivity sector—is reflected in rising per capita product in the economy. Under modern conditions of development this is reinforced by the advantages of delayed industrialization inasmuch as the process of industrial expansion in a country such as China is accompanied by discrete leaps in technology. Within this context, even the postwar dismantling of Manchurian plants by the Soviets may be considered a blessing in disguise, from the Chinese point of view. Plant installations built by the Japanese that were removed in 1945 have by now largely been replaced by more up-to-date Soviet equipment. The same applies to plants newly built; for instance, on the basis of eye-witness and other accounts, a flax mill located near Harbin and completed in 1952 seems to incorporate the advances of modern technology. Similarly, it may be assumed that the two new steel mills to be built in China by Soviet engineers with Soviet equipment will be based on modern Russian methods of steel-making—methods which do not seem to lag far behind those applied in the United States.

The same may be put differently—i.e., high rates of investment in an economy in which the capital base is small will necessarily lead to rapid transformations in the age-composition of capital. Thus, in our model, aggregate stock would be almost doubled,

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while industrial capital would be quadrupled, during the ten-year period. By 1962, practically all of the industrial capital will have been installed during the preceding decade, while for the economy as a whole only one-third of the stock will still be old.

Of course, this process of rejuvenation accelerates economic growth not only through its technological effects, but also by virtue of the fact that as new capital displaces old, the share of replacement allowances in the investment total declines. This tendency is reinforced in countries such as China, which concentrate upon the development of producers' goods industries with a longer life-span for equipment and therefore relatively lower rates of capital consumption.

In working with aggregate models of this type, the problems of structural adjustments are frequently overlooked. In the Chinese case, three types of bottlenecks may be ultimately limiting: the rate of net resource transfer out of agriculture, the terms and conditions under which China can import the wherewithal of economic development, and the level of technological and entrepreneurial skill available in the economy.

As was pointed out earlier, in order to satisfy the rising demand for farm products implicit in Table 12, agricultural output would have to increase by about 26 per cent within the decade. It was also concluded that even if such an increase were technically feasible within the narrow limits of current agricultural investment levels, it is rather doubtful, in the face of prevailing Chinese Communist farm policies, that such increases can be attained. Actually, given the general character of the regime, its policies, and its objectives, it may be expected to bend every effort to maintain the level of net resource transfers out of agriculture even with stagnating or declining agricultural production. The extent to which this can be accomplished without seriously undermining the stability of the regime is necessarily one of the most critical variables affecting the rate of growth.

Similar limitations to growth and industrial development may be imposed by restrictions upon Communist China's ability to import raw materials and capital goods. It was previously indicated that in 1952 Communist China's imports from the Soviet Union constituted about 1 to 2 per cent of Soviet industrial and

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mining production. Assuming that there is no relaxation in free-world trade controls, that China continues to obtain about half her imports from the USSR, and that economic growth in China proceeds at the rate projected in our model, while in the USSR it continues at an estimated rate of from 6 to 7 per cent a year, this percentage may have to be maintained. Such a close dependence upon the Soviet Union and the Soviet bloc not only may yield terms of trade unfavorable to China, but is almost certainly bound to result in specific bottlenecks in deliveries of capital goods—that is, goods for which the demand is high in the bloc itself. On the other hand, should trade controls be lifted or significantly relaxed, China's import position would be greatly relieved.

A highly centralized administration of an economic development program in a vast country such as China requires a huge bureaucratic apparatus with a wide variety of skills. There is need for engineers, technicians, foremen, skilled workers, technical and economic planners, statisticians, cadres to administer the economic controls, rural extension agents, managers of plants and vast state enterprises, etc. While some of the gap in trained manpower is to be filled through Soviet technical assistance and through greatly expanded domestic training facilities at the secondary school rather than the advanced university level, shortages are likely to remain acute throughout the decade. This, in turn, will tend to exercise a depressing effect upon growth, through inefficiencies, errors in economic and industrial management, and low productivity.

While recognizing the fundamental impact of these structural bottlenecks upon the prospects for economic growth in Communist China, it is essential not to overemphasize them. These bottlenecks present no absolute barriers, but only additional roadblocks. This means that in order to maintain a high rate of growth (e.g., from 3 to 4 per cent) under such circumstances, the rate of resource mobilization must be raised above the levels that would be required in the absence of such bottlenecks. It may be said on a priori grounds that a regime which is capable of channeling 20 to 30 per cent of national expenditure through the government budget is likely to be able to sustain a comparatively

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rapid tempo of economic and industrial expansion—somewhat more rapid in the absence of serious structural problems and less rapid in the face of such problems—unless all or most of these resources are dissipated in current military expenditures and in the costs of administering the controls required to mobilize this share of current output. Finally, in appraising the plausibility of a 3 to 4 per cent a year rate of growth for Communist China, the Japanese experience may be most relevant. Meiji Japan, starting from roughly the same stage of backwardness as mainland China on the eve of its first Five-Year Plan (1952), and with a resource base that was certainly no more favorable, moved ahead at an estimated rate of from 4 to 5 per cent between 1880 and 1900.[11]

While a number of factors seem to point to comparatively rapid rates of prospective growth in Communist China, per capita gross product in 1962 would still be only around US $60, in spite of the pace of expansion envisaged in our model. However, the share of industrial production in total national product would rise from 7 to 16 per cent. The economy of mainland China would thus attain a stage of development which would still definitely rank it among the underdeveloped areas of the world. In the context of Asia, however, growth at such speed would constitute a most impressive performance. Depending on the course of events elsewhere in Asia, a development of this kind in Communist China could significantly affect the regional political and power balance.

[11] The term "resource base" is used here in its broadest sense. Thus, China is comparatively better endowed with mineral resources, while Japan faced an expanding world market and a different international climate. For further details, see Table 7 and Section IIIA.

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Review Articles

ECONOMIC DEVELOPMENT: Principles and Patterns

By EVERETT E. HAGEN

Harold S. Williamson and John A. Buttrick, eds., *Economic Development: Principles and Patterns*, New York, Prentice-Hall, Inc., 1954, 576 pp. $9.00.

Harvey Leibenstein, *A Theory of Economic-Demographic Development*, Princeton, N.J., Princeton University Press, 1954, 204 pp. $4.00.

I

WHEN the three blind men felt different parts of the elephant's body, they came to radically different conclusions about its nature. Social scientists have come increasingly to realize that unless there is integration among the sciences concerned with human behavior—sociology, psychology, psychiatry, cultural anthropology, economics, political science and, in some contexts, demography (regarded as a branch of sociology)—their various comments concerning any given problem may resemble those of the three blind men. Yet the formal framework for such integrated study hardly exists.[1]

Rapid steps in the development of an integrated theory will probably come when two conditions exist. First, the separate disciplines must have reached a stage at which all, or at least most of them, have a coherent system of analysis, rather than merely a series of insights. Second, they must be facing a common problem which forces integrated consideration. One field in which of necessity this integration may occur fairly rapidly in

[1] In *Toward a General Theory of Action*, Parsons, Shils, and others presented such a framework for the integration of sociology, psychology, and cultural anthropology (Talcott Parsons and Edward A. Shils, eds., *Toward a General Theory of Action*, Cambridge, Mass., 1951; see a review article by W. W. Rostow in the July 1953 issue of this journal). Whatever may have been their degree of success in this attempt, they ignored economics and political science. In his *Process of Economic Growth* (New York, 1952), W. W. Rostow presents such an interdisciplinary framework for the study of economic development. However, his comments concerning non-economic factors lay no claim to being more than sketchy.

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the near future is in the study of the economic development of "underdeveloped" societies.

Consequently, the two books under review are of especial interest. Both attempt interdisciplinary examinations of the problem of economic growth, one by bringing together a group of collaborators, the other by an examination by a single author of the relationships between two disciplines. The chapter headings in the volume edited by Williamson and Buttrick include, in addition to a number of economic topics: demographic patterns; entrepreneurship and technical change; the migration of capital, people, and technology; cultural factors; and the role of government.

II

The writer is trained primarily in economics; of the other social sciences he is merely an observer, albeit an extremely interested one because of his concern with the problem of economic growth; and he writes this essay since an ideal reviewer is not available. A summary statement will be presented first of the contribution which it seems to the reviewer each of various social sciences must make to an understanding of the problem of economic growth. Against the background of this statement, the contributions of the two books will be appraised.

To many economists, the problem of economic development has seemed primarily a problem of obtaining enough resources for capital formation and of allocating them wisely. That shortage of resources for capital formation has not always been the limiting factor in economic development is indicated by the fact that in many low-income countries, over long periods of time, fewer resources have been used for capital formation than have been available. Because of lack of knowledge, inadequate motivation, or obstacles inherent in the social structure, such countries have increased their capital at a slower pace than their own resources would have made possible. Nevertheless, even if the other obstacles are overcome, capital shortage may be a crucial factor, and a theory of economic development will be deficient without an analysis of the factors determining capital requirements, the

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ability to meet them, and the principles which should guide the allocation of new investment within the economy.

Two major attempts have been made to derive a quantitative estimate of capital requirements for economic growth, one by Colin Clark in his *Conditions of Economic Progress*,[2] and the other by the group of experts who prepared a report for the United Nations on *Measures for the Economic Development of Underdeveloped Countries*.[3] Both attempts, however, contain important methodological defects. Concerning the problem of optimum allocation of investment for economic growth, present economic theory is also inadequate—as will be noted in Section III below. But capital formation is not the central process in economic growth: it is only one aspect of that process. Almost by definition, economic growth consists of the introduction of new methods, and its central problem may be said to be the problem of innovation, if the term "innovation" is used in a broad sense. A loose distinction may be made among three kinds of innovation: social, entrepreneurial, and innovation in the small.

The importance of business entrepreneurship as an agent of innovation in productive methods is fully recognized in the social sciences, perhaps most of all in economics, where the late Professor Schumpeter was its apostle; but cultural anthropology has not evolved a satisfactory theory of its appearance in the culture of a society.[4] In pre-industrial cultures, to repeat a convenient stereotype, status rather than activity gives inner satisfaction and yields prestige; and, specifically, positions in the service of the ruler, in religious office, as landowner or as scholar, are those which have the greatest honorific value for most of the population. There is a general aversion to the grubby business of managing a productive enterprise. In present-day Western culture, on the other hand—and perhaps this was even more true of nineteenth-century Western culture—few activities yield more prestige or give more satisfaction to a majority of social leaders than the function of managing the men, the machines, and the mate-

[2] Rev. ed., London, 1951.

[3] United Nations, Department of Economic Affairs, New York, 1951.

[4] H. G. Barnett, *Innovation*, New York, 1953, represents an initial attempt to formulate such a theory.

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rials which enter into a productive enterprise so that it "hums" and grows.

How does such a change come about? Even for Western Europe, historical theses vary, from the one that respect for the businessman grew out of the Protestant ethic, to the opposing one that the Protestant ethic arose out of the respect for the "businesslike man"—that the Europeans who conducted the Industrial Revolution created God in their own image. Whatever may be the true explanation, it is certain that the cultures of presently underdeveloped countries will not follow the path of Western Europe. It is equally certain that the satisfaction and prestige yielded by innovational business entrepreneurship must increase greatly in underdeveloped economies if they are to progress technologically. How may such a new element enter a culture? What will determine the speed of the process? How will it interact with existing institutions? At the present, we know little and theorize less concerning this topic.

For maximum progress in economic welfare there is also needed motivation, widespread in the culture, toward "innovation in the small." The majority of workers engaged in productive activities on the farm, in the shop, or in the factory must be in some degree "gadgeteers." There is need for a general receptivity to technological change in the small. How does this disposition develop? Certainly, one of the elements—to use another stereotyped phrase—is a transition from traditionalism to rationalism. Assuming that in some miraculous way the leaders of the culture become autonomous agents and can elect to foster rationalism and a general interest in technical innovation, how are they able to do so? What other elements are involved in the development of widespread motivation toward innovation in the small?

At the other end of the scale, motivation to social innovation is needed. Any number of social institutions must change in the process of economic development. To cite some random examples, these may include the joint or extended family, caste, inactive land ownership, legal forms of property ownership, the traditional round of village life, methods of business organization and administration, forms of political leadership, and methods of governmental administration. Analysis of the speed with

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which and the way in which these changes take place, and the degree to which they modify and in turn are modified by technological change, is an essential ingredient in a knowledge of economic development.

The economic purpose of economic development, at least in democratic societies, is an increase not merely in aggregate output and income, but in income per capita. If growth of the economy is accompanied by an equally rapid rate of population expansion, the benefit of the growth will be nullified.

What forces determine population trends? If progress in sanitation and health care reduces death rates, under what circumstances will birth rates also decline? It was generally thought at one time that the causation was rather narrowly economic—that family size rose as income rose, unless (and until) a new higher standard of living became desired by the population, which would lead to a reduction of births and a new stabilization of population size.

It now seems that this theory of population change is quite inadequate. Urbanization as such, various cultural changes quite apart from urbanization, literacy simply in itself, perhaps merely a decline in death rates—because many births are less necessary to preserve the family—all of these may affect population trends. Which of these determinants are important? And how do they operate?

Our knowledge concerning many of these factors is slight. Thus, all of the social sciences have advances to make. Further, there are many interrelations to establish. Those between cultural change and population change have just been mentioned. The speed of development of entrepreneurship will surely be affected by the types of enterprises which are established by new investment, i.e., by the allocation of investment; but the nature of such influence is not clear. As economic development proceeds, the rate of saving increases, but the weight of various cultural changes in inducing the increase is uncertain. Whether the rationalistic viewpoint can be induced in village life, or whether it depends on urbanization, should importantly affect plans for investment allocation. But while there is some reason to question the earlier assumption that rationalism simply will not develop

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without urbanization, we do not know enough about the process of cultural change to be sure of the pertinent relationships. These are examples of the many interrelationships which might be cited.

We want to know not only the effect of psychological, social, political, and demographic changes on economic development, but also many reverse effects. What, for example, are the effects of economic change on psychological states? Some types of economic change which are a necessary part of economic development force changes in ways of living. But "our individual beliefs and attitudes, our way of life, are not something we easily cut off. They are our personal means of solving our problems. . . . When any of us is confronted by . . . a new situation with its new demands for which we have no immediate answer, we become insecure in varying degrees."[5] This insecurity tends, at least in some contexts, to cause erratic and aggressive action or blind devotion to a leader who meets the emotional need. Is such insecurity less if the changes are made by and on behalf of a protective government? Is this one explanation of the major role played by governmental activity in present-day economic development? If some patterns of economic development are less conducive to the development of emotional insecurity than others, which ones are they? And what degree of influence do they exert?

Only very recently have the social sciences begun to direct their attention to these and the many other questions which might be asked. The demographers have been too occupied with the important task of data collection and analysis to do much theorizing. Psychiatrists have been too busy analyzing the individual to elaborate their theories about unconscious motivations of societies. The cultural anthropologists have until recently devoted their primary attention to the structure of societies rather than to processes of change. When sociologists study societies other than their own, their work of course is closely related to cultural anthropology. They have not contributed to the literature of cultural change. Economists have had interesting things

[5] Dr. William C. Menninger, *Social Change and Scientific Progress*, Arthur Dehon Little Memorial Lecture, Massachusetts Institute of Technology, Cambridge, Mass., 1951, pp. 29-30. I have reversed the order of the sentences quoted.

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to say about the process of growth, but they are brilliant insights, not part of the central body of theory.[6]

The central body of economic theory is in two ways peculiarly inapplicable to the study of economic growth. First, it is for the sake of simplicity based upon the assumption of a constant state of the arts, i.e., unchanging technical knowledge, whereas almost by definition the process of economic development is the process of change in techniques. Second, economic analysis of the optimum allocation of productive resources has until recently assumed that change was occurring at only one point in the economic system; analysis has been made of the principles governing optimum allocation at this point, all other conditions in the economic system remaining unchanged. This marginal analysis is a powerful tool, but like the assumption of a constant state of the arts, it is peculiarly inapplicable to the analysis of economic growth, in which large changes, not small ones, must be analyzed, and in which the advantageousness of change at any given point in the economy will be influenced by changes which are occurring simultaneously throughout the economy.[7]

Yet in spite of their preoccupation until recently with other matters, the various social sciences do have important and relevant things to say about economic growth, and perhaps the time is ripe for the rapid development of a body of theory.

III

The collaborative volume edited by Williamson and Buttrick is designed primarily as a text or a survey of present knowledge, not as a research monograph, and should not be expected to pierce existing frontiers. The last three chapters, constituting slightly more than one-fifth of the book, are studies of the population problem in Japan and Korea and of economic development in India and Mexico by scholars who have made studies of these countries. They are useful case studies. The ten preceding chapters, whose content has been suggested in Section I, are also

[6] The economist who has written most about the process of growth, Joseph A. Schumpeter, has glorified the entrepreneur.

[7] P. N. Rosenstein-Rodan set forth this fact clearly in his "Problems of Industrialisation of Eastern and South-Eastern Europe," *Economic Journal*, LIII (June-September, 1943), pp. 202-11.

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competent. Many readers will find one or all of them of considerable value. The authors' expertise, however, is in the topics discussed rather than in underdeveloped economies or economic development as such. This characteristic accounts perhaps for the nature of a number of the essays. In several chapters, the authors present in the main general principles derived from their general professional knowledge, and by a process of deduction arrive at conclusions about economic development.

A conspicuous example of this tendency is Chapter 5 on the "Formation of Capital," by John A. Buttrick. The first three-fourths of its sixty pages consists of an analysis, at the level of an elementary or sometimes an intermediate text, of the proper level of investment in a theoretical model, of the relation of the marginal propensity to consume, the multiplier, and the accelerator to the model, and of the relationship of capital formation to cyclic fluctuations in economic activity. Of the remaining one-fourth of the chapter, part refers to a hypothetical underdeveloped country, defined in purely economic terms. However, the assumptions about the economic nature of underdeveloped countries are in important part erroneous, and the conclusions drawn from the theoretical model concerning the countries are not very illuminating. For example, the assumption that a given amount of investment creates more productive capacity in a typical underdeveloped economy than in an advanced one is certainly erroneous; and the conclusion that this effect of investment is the reason for the existence of underemployment or hidden unemployment in underdeveloped countries is certainly incorrect and irrelevant to the problem of underdeveloped countries in the real world. It should be added, however, that these pages do include a realistic discussion of the problems of using unemployed workers in underdeveloped countries.

These strictures concerning the undue use of deduction from general premises apply in lesser degree to most other chapters of the book. They are inapplicable to the introduction by Professor Williamson, to Chapter 6 on "Entrepreneurship and Technological Change," by Yale Brozen, and to Chapter 9 on "Cultural Factors," by Francis L. Hsu. J. J. Spengler's discussion of "Demographic Patterns" is a useful summary. Furthermore, it would

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be unfair to omit mention of the shrewd observations and stimulating suggestions contained in other chapters. It remains true, however, that such empirical basis as there is for the generalizations advanced is mainly "casual empiricism." Except for the three chapters presenting country studies, illustrative data in the book apply much more to that singularly inappropriate area, the United States, than to any other country.

The reviewer is convinced that if a fully pertinent body of theory is to develop, deductions from currently accepted generalizations in the various social sciences must be met by a strong stream of induction from observation of current experience or by interpretation or reinterpretation of the history of "underdeveloped" and developing societies. This is especially true in the field of economics. It is a weakness in this book that such induction is too little in evidence.

IV

The chapter by Brozen, drawing judiciously from the work of others as well as from his own studies, discusses the role of entrepreneurs in economic change, the conditions under which human energies are apt to flow into the entrepreneurial function in productive enterprises, the role of government in stimulating innovating entrepreneurship, and the techniques in economic growth which are most conducive to the emergence of a continuing sequence of development. The chapter presents not one integrated theory, but a series of judicious observations.

Hsu finds Western material progress to be rooted in insecurity of the individual. Taking China as typical of the East, he writes: "The Chinese culture pattern has as its central theme what may be described as mutual dependence among men. . . . Since personal success or failure is relatively unimportant to their basic security because their anchorage among fellow men is firm and unshakable, the Chinese tend to lack the determination and the desire to grind forth toward remoter goals" (pp. 327, 331). Under Western individualism, "the Westerner's success is individual, and his triumph is great; but his failure is also his own and his misery extreme. . . . the Westerner must have success because

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he has no retreat" (p. 330). Loneliness is inevitable, and the greater scientific achievement of the West is due not to more rational behavior—the West is not more rational than the East—but to this loneliness. "This Western loneliness, though sometimes terrible in its consequences on the mind of the individual, is the fertile field which has nourished Western imagination, which in turn is the basis of Western science and invention, the most essential ingredients of economic growth of the Western type" (p. 336).

Hsu's prescription for stimulating economic development therefore runs as follows: (1) Set up needed public works projects, such as a dam or irrigation canal. The work opportunity offered will draw a labor force from existing underemployed areas. (2) Initiate a mild inflation to create insecurity among the middle and upper classes. (3) Modify the tax system so that it favors vertical or horizontal occupational movement by individuals. (4) Publicly honor economic achievement. (5) Increase the supply of new types of consumer goods. (6) Do not expect education to be helpful until the process of economic development has been initiated by the means indicated.

In the writer's judgment, Hsu overstates the effects of emotional insecurity in Western culture. Important though it is in explaining Western behavior, it alone is not a convincing explanation of the range of Western scientific and material achievement. The complex of qualities in Western culture which can be grouped under the heading of rationalism fosters economic advance through a number of other channels rather than merely through the development of insecurity. Hsu's analysis does not attempt to explain the emergence of rationalism in a culture. His prescription for inducing progress is a bit thin, and in part inconsistent with his premises. Yet his analysis is the sort of stimulating probing which is a necessary step in the development of adequate theory.

V

Leibenstein's book, in contrast to that edited by Williamson and Buttrick, is a research monograph. It sets forth an analytical

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model stated in mathematical terms[8] which portrays the influence of income changes on population size. While the exercise purports to be only one in abstract theory, and Leibenstein warns of the difference between a theoretical model and reality, he nevertheless draws a number of conclusions, with the implication that they are of importance for the real world.

Leibenstein elaborates and manipulates the model with great dexterity and ingenuity. In addition, he presents chapters on the concepts of statics, dynamics, and equilibrium and on the process of model construction, and a chapter on the relevance, nature, and scope of optimum population theory which presents a useful critique of that theory.

The model assumes an initial Malthusian equilibrium. Average per capita income is low enough so that the population just sustains itself in size. There is no net saving or investment. The main pieces of apparatus used to manipulate the model are:

(1) Innovation, emigration, or the contribution of a lump of capital from outside the system will cause an initial increase in per capita income. There will then be net saving and net capital formation. If the population size does not increase, capital formation will progressively increase per capita income in subsequent time periods.

(2) However, the increase in per capita income will cause increase in population size, which in turn will tend to decrease per capita income. The two opposing influences on per capita income will then have one of three effects: (a) If the effect of population increase outweighs that of capital formation, per capita income will fall to the initial level, and population will cease to increase. But the new equilibrium will be at a larger population size than the old. (b) If the effect of capital formation

[8] The mathematical presentation is marred slightly by two errors. On page 45, Leibenstein states that in his simplest model the size of the population is a function of average per capita income. This is incorrect. The model is actually one in which population size is a function of aggregate income; aggregate income is a function of population size and the quantity of resources; and the quantity of resources is given. Again, on page 47, he states that $B = \frac{G_t}{i_{t-1}}$, and that $M = (1 + AB)$. One of these definitions is incorrect. Either $M = AB$, or $B = \frac{G_t - g_{t-1}}{i_{t-1}}$. But though the careless definitions may cause slight momentary confusion, the subsequent mathematical treatment is correct.

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is the greater, per capita income and population size will rise indefinitely. (c) In the borderline case, where the two effects are just equal, after the initial increase, per capita income will remain at the new level, with population size increasing indefinitely.

There is, however, a complication to be considered. (d) Even if the population effect outweighs the capital formation effect, after a small initial increase in per capita income, this may not be true after a sufficiently large initial increase. For there is a biological upper limit to the birth rate (and a biological lower limit to the death rate), and a sufficiently large initial increase in income (or series of initial increases) will bring about such a high rate of capital formation and hence cumulative increase in aggregate income that it will outweigh even the maximum rate of population increase. In that event, population and per capita income will tend to increase indefinitely. Further, it may be assumed that, above a certain per capita income level, the rate of population increase will slow down, and a large enough initial increase or series of increases in income will bring per capita income up into this range.

In his discussion of what this model implies, Leibenstein curiously enough omits all reference to case (b), which seems of considerable practical relevance. Instead he concentrates on case (d). This leads him to stress the importance of the initial increase in income, since the question of whether there will be a continuing increase or a reversion to the original per capita income depends solely on the size of the initial increase.

It is easy to point to the limited nature of this analysis. The success of economic development does not depend upon reaching an initial rate of income increase which it is beyond the power of the maximum possible rate of population increase to counteract. Nor does the rate of population increase slacken simply because a sufficiently high level of per capita income is reached. Leibenstein sometimes forgets the restricted nature of his assumptions, and writes as though they reflected all of the major relevant conditions in the real world. He makes only passing reference in one brief passage to (non-economic) cultural influences on population size. Yet these, and not merely biological factors or rises

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in income, set limits to rates of population growth and make a permanent increase in per capita income possible. Still, the idea of the minimum critical initial effort in economic growth is important, and Leibenstein's analysis is pertinent and suggestive.

These two volumes do not carry one far along the path toward the development of an interdisciplinary theory of economic growth. They are perhaps more important as symbols of the direction in which we must travel than for their substantive content. But they are steps in the direction in which progress lies.

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TOWARD GREATER ORDER IN THE STUDY OF INTERNATIONAL POLITICS

By RICHARD C. SNYDER

Charles P. Schleicher, *Introduction to International Relations*, New York, Prentice Hall, Inc., 1954, 941 pp. $6.95.

Norman J. Padelford and George A. Lincoln, *International Politics: Foundations of International Relations*, New York, The Macmillan Company, 1954, 719 pp. $6.50.

Norman D. Palmer and Howard C. Perkins, *International Relations: The World Community in Transition*, New York, Houghton Mifflin Company, 1953, 1270 pp. $7.00.

Robert Strausz-Hupé and Stefan Possony, *International Relations in the Age of the Conflict Between Democracy and Dictatorship*, 2nd ed., New York, McGraw-Hill Book Co., Inc., 1954, 826 pp. $6.50.

Hans J. Morgenthau, *Politics Among Nations: The Struggle for Power and Peace*, 2nd ed., New York, Alfred A. Knopf, 1954, 600 pp. $5.75.

THESE five recent and representative introductory textbooks provide the basis for some remarks on the study of international politics.[1] It should be clearly understood, however, that this is not a full review of the individual works but, rather, a commentary on the literature they represent. Each book reflects a prodigious and competent scholarly effort and it would be manifestly unfair to have anything that is said here interpreted as an adequate appraisal of their total value.[2] Furthermore, this essay is limited to *one* common aspect of the literature, namely, the use of certain concepts for purposes of organizing, describing, and interpreting a given range of phenomena.

I

There are marked similarities in both subject matter and concepts among these authors. With a few exceptions, most of the descriptive material could be fitted without distortion into crude traditional categories: the state system, elements of national

[1] It is not implied that these are the only useful or influential texts. The particular choices happened to be recent publications which the writer had carefully analyzed.

[2] A more detailed analysis of the contemporary literature of international politics will be published under the title, *Approaches to the Study of International Politics* (Princeton University, Foreign Policy Analysis Project, Publication No. 6), by the present writer, Burton M. Sapin, and H. W. Bruck.

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power, conduct of foreign policy, diplomacy, policy techniques and strategies (in general, and those of the major nations), nationalism, imperialism, sovereignty, international conflict (peace and war), pacific settlement, international organization (the League of Nations, the United Nations, and the world community), international law, and critical periods in the history of international relations. Also shared in noteworthy measure is a common language of technical discourse which *appears* to be built around these key terms: *politics, power, state, national interest, conflict, war, peace, policy, cooperation, technique,* and *objectives.*

Power, conflict, and peace are among the central concepts—using the word "concept" loosely to include analytic foci—which are used in some manner and degree by all the authors. The five books give at least one chapter to a specification of the "elements of national power" (ordinarily geography, population, technology, natural resources, morale, political institutions, and leadership). Quantitative indices as well as qualitative judgments of these elements become tools for estimating a state's capacity to defend itself, to entertain certain kinds of objectives, and to utilize particular techniques for the pursuit of peacetime aims. States are classified and compared in terms of their relative power position. National power is thus viewed as means and as a limitation on state action. The necessity for power is recognized as a paramount motivational factor. "Balance of power" is conceived as policy, as a condition, and as a form of general equilibrium analysis.

Though the authors differ importantly in the role played by the power concept and power factors in their presentations, all stress this dominant theme. Schleicher's analysis shows less of an all-pervasive influence from this source than the others, while Morgenthau's comes closest to embodying a straight power approach. Closely related to the power concept as a focus is an emphasis on patterns of conflict. Differences notwithstanding, there is a strong preoccupation with the types and causes of conflict between states. Conflict is almost *the* typical mode of state interaction. Emphasis on power and conflict is naturally accompanied by an equal concern for peace. The problem of peace is a

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matter of curbing, channeling, and abolishing forms of power and of minimizing conflict. Thus international politics are deemed to be in fact, and should be interpreted as, primarily concerned with power and conflict and their control.

Differences among the authors are just as marked as similarities. They do not agree on a definition of international politics—on the scope of this field of study. Schleicher includes all interstate relations, although only some of these are *political* as he conceives the term (p. 31). Padelford and Lincoln define international politics as the *interaction of state policies* within changing patterns of power relationships (pp. 4, 6). For Palmer and Perkins, the study of international politics is essentially concerned with the *state system* (p. 35). On the other hand, Strausz-Hupé and Possony seem to include in international politics the actions of citizens and the decisions of politically significant private groups, although it is clearly stated that the most important decisions and actions are those made by governments in the spheres of economics and power (p. 4). Morgenthau focuses his analysis on *political relations* (p. 14) and on the problems of peace (p. 20). Political relations are power relations and not every governmental act is a political act. Thus there are broad and narrow ranges of phenomena included for description and explanation. The criteria for inclusion or exclusion obviously relate to the authors' purposes and to their basic assumptions about the nature of social action generally, but such criteria are not made explicit.

These differences in scope are paralleled by, and intermixed with, even greater differences in method or analytic schemes. Schleicher builds his frame of reference around an action concept and a typology of state interaction. The four components of action are: actors, objectives, conditions, and resources. The four major types of interaction are: competition, conflict, cooperation, and accommodation. Padelford and Lincoln's approach is oriented toward policy and policy-making. Concentration is on the elements and sources of foreign policy, on national interests, on policy formation, as well as on the "projection" of competitive policies into the international realm and its consequences. Palmer and Perkins employ as their organizing de-

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vice the central notion of the state system which determines "the pattern of international life" and has three essential features: the doctrine of nationalism, the concept of sovereignty, and the principle of power. In the case of Strausz-Hupé and Possony, there is a primary emphasis on the "instrumental" aspects of state behavior—the nature, use, and bases of effective foreign policy techniques—but their analytic scheme consists of a conflict-cooperation continuum within which international relations are viewed as fluctuating. Morgenthau's two fundamental concepts are power and peace. International politics is the struggle for, and use of, power among nations—a postulate based on a general "realist" theory of politics consisting of six principles, one of which is "the concept of interest defined in terms of power." Balance of power is an important sub-concept.

II

There is much to be pleased about in these introductory texts. The trend toward greater coherence and explicit conceptualization is especially noteworthy. Schleicher in his introduction invites the student to become a social scientist and his first chapter is entitled, "How to Think about International Relations," followed by a later chapter on the nature of international politics. Part One of Strausz-Hupé and Possony includes three chapters which outline a "Theory of International Politics." Padelford and Lincoln give attention to the development of concepts in a chapter analyzing the nature of foreign policy. Morgenthau begins his volume with two chapters entitled, "A Realist Theory of International Politics," and "The Science of International Politics." "Science" and "theory" are now common words. One welcome by-product of greater concern for systematic analysis is more adequate and fruitful definition of terms and attempts at useful typologies. Another characteristic of these books is the increasing acceptance of policy-making as a significant analytic focus—most clearly evident in Schleicher, Padelford and Lincoln, and Strausz-Hupé and Possony. Not only has "foreign policy" been clarified as a term and a subsidiary concept, but more space is being devoted to the ingredients of decision, to

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decisional processes, to the tasks of the policy-maker, to the kinds of considerations which enter into decisions, to means-ends relationships, to limitations on policy choices, and to problems of execution. Finally, mention should be made of the open acceptance of the study of international relations as dependent on several disciplines. In its own way, each of these texts shows evidence of an awareness of, and some reliance upon, the relevant writings of the other social sciences, history, and philosophy.

III

Despite the progress cited above and despite the obvious value of these works individually, certain problems and weaknesses must—in all candor—be faced. It would be manifestly unfair to ask perfection of the authors even within the limits they have set themselves, and it would be unfair to chide them for not doing things they had no intention of doing. Nor could one reasonably expect that the authors would carry on esoteric theoretical combat at the expense of introducing the student to the subject. Logical and other imperfections can be found in all books of great sweep and magnitude. It takes not one whit from the contributions and accomplishments of these five books to raise questions which may be important for political analysts concerned with state behavior.

The search for more adequate and fruitful categories and concepts with which to identify, organize, describe, and interpret social phenomena is an ever-present feature of systematic investigation. That the search can be confused and fruitless is undeniable. That it must be undertaken is also undeniable. Nothing is so easy as fruitless and frustrating debate on the subject of suitable categories and concepts. If the last fifteen years in political science are any indication, nothing is so difficult as keeping this enterprise rigorous, tough-minded, and subject to precisely stated purposes. Even professional scholars find it easier to apply concepts than to analyze their qualities. Yet the general problem of conceptualization would seem to be basic to the results we hope to achieve.

There are certain conditions which should undergird efforts

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to establish and improve the major categories and concepts in any field of learning. First, before conceptual tools are chosen or fashioned, the basic queries which are to direct investigation or study ought to be formulated clearly and meaningfully. What do we wish to know? Are the questions general or specific? How much do we wish to know? Do we want descriptive or predictive propositions to result from our inquiry? With some exceptions, the books discussed here do not systematically present such questions. To define the study of international relations as, say, the study of specified interactions among units called "states" does not automatically provide fundamental questions, since only a small number could be derived from that formulation without further specifications. These questions play a vital role in determining the range of phenomena to be included in investigation. Schleicher says he wants to know *why* states behave as they do. This is a far cry from asking: *How* do states interact with each other? In the first case, the main question leads to others: Why do policy-makers have the general goals they do? Why do policy-makers see some situations as threatening? as acceptable? as demanding? as routine? In the second case, ensuing questions are: By what institutional devices do states carry on their mutual relations? What are the customs and procedures of normal diplomatic intercourse? "Why" questions reach for causes, for explanations. "How" and "what" questions require varying degrees of description. One class of questions necessitates analysis of antecedent conditions and determinants with respect to any action or event. The other class centers on the substance of events and actions and on the patterns of relationship among them.

Second, part of the task of defining a field is to characterize as well as to limit the phenomena to be observed and explained. To say that the study of international relations includes *all* the actions, interactions, and relationships between societies does not tell us what is to be meant *analytically* by "action," "interaction," and "relationship," and does not specify for us what the distinguishing characteristic of the empirical events is for the political analyst—in other words, it does not specify what is *political* and what is *politically relevant*. Presumably, concepts help in this task by classifying phenomena and assigning them certain

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properties. The attempt to use power as the concept which enables us to determine what is a political action, interaction, or relationship should be noted in this connection. Very often matters discussed under "nationalism" could be and sometimes are discussed under "ideology." Thus "Manifest Destiny" may be viewed as an attitude or a spirit. It may also be viewed as an idea or as a set of related beliefs. Since ideas, if they are important determinants of action, are accompanied by some motivational pull and since attitudes usually have some object which serves to activate people, it is difficult to separate the two aspects. Unless "nationalism" and "ideology" are precisely defined, the two concepts tell us little about the phenomena which might be classified under each. Categories are linked to (and often are confused with) concepts but essentially perform only the classification function. Often categories lose their criteria of classification and often the criteria are ambiguous, and thus the categories degenerate into mere labels. The danger lies in the illusion that by labeling something, we have either said something significant about it or explained it. By classifying, we hope to separate phenomena and aspects of phenomena for some useful purpose. Concepts help explain what we have classified. Slipshod categorization hinders analysis, because it confronts the investigator with multiple choices of interpretation without any clear guide to selection.

Third, concepts should be formulated with due consideration for their logical rigor, their operational quality, their limits, and their relation to the phenomena with which they are supposed to deal. Fourth, categories and concepts ought to be examined regularly and tested by a number of criteria—among them, fruitfulness in research and increased understanding. Concepts are only tools and they should not be allowed to freeze any investigative enterprise. Through carelessness, repetition, and misuse, concepts may lose their identity as analytic devices and become "true" or accepted as "things."

Conceptual difficulties continue to beset the field of international politics. Central concepts appear to be either undefined, imprecisely defined, or defined so broadly as to lose analytic strength. Examples of the latter difficulty are peace, cooperation,

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objective, conflict, and power, for example. Thus it is difficult to find referents for the concept of cooperation. Is it a *property* of certain acts? Is it a *chain of related actions*? Is it a *type* of act? Does it pertain to the rules governing the interaction between two or more states? Does it refer to ends? to means? to the consequences of behavior patterns? To say that cooperation is marked by the absence of conflicting aims and by the pooling of resources does not really answer these questions. If nation X and nation Y pool their resources in spite of conflicting aims, is this cooperation? Often cooperation is spoken of as if it were an "amount" of something and as if it were always a "good" thing. In the conflict-cooperation continuum, does "more" of cooperation imply "less" of conflict? If perfect cooperation is the absence of conflict and if politics is defined as par excellence power conflict, is a cooperative relationship also apolitical or non-political? Is cooperation the absence of conflict or a system for channeling or reducing or accommodating conflict?

"Conflict," too, is a troublesome term as currently employed. Does conflict refer to a *state of affairs*? to a *type* of interaction? to a *process* of interaction? to an *aspect* of action or of a relationship? to *differences* of objective, technique, and need? to *competition* for the same end? to the *fact* of competition, or its *outcome* or *resolution*? to *limited satisfactions* available for given goals? If power is an urge and if power-seeking automatically produces conflict, how can conflict be avoided? Conflict is alternately defined as *action, policy, attitude,* and *relationship*. Analyzing the cause of conflict and breaking it down into classes do not satisfy the necessity for specification of some analytic property which can be identified empirically.

It is possible to use such terms in their common-sense meanings, i.e., the meanings they have in the everyday world. The trouble is that to ensure communication, multiple meanings or shades of meaning come to be attached to such terms. This usually raises few difficulties in ordinary verbal discussion and understanding, but if systematic analysis and research are desired, the common-sense terms must either be clarified or replaced. If they are replaced *by identical words* so that the common-sense concept and the scientific concept are indistinguish-

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able, the ambiguities of the former tend to be built into the latter.

Another question which arises is whether the preoccupations, problems, and concepts of the everyday world should be permitted to determine which scientific concepts and systematic categories are most fruitful—particularly in explaining events, as distinct from describing them. Peace may be an important practical problem, but it is questionable whether the concept of peace can be fashioned into a major analytic tool. Conflict may be a familiar phenomenon in the real world, but whether the actions, interactions, and relationships referred to are best handled analytically by a concept of conflict is another matter. It can be argued that the most effective way to understand what people want to understand about peace and conflict is to employ conceptual tools in which the terms "peace" and "conflict" do not appear. On the other hand, it may be argued that multiple components or meanings of concepts are often useful and/or required. Then it is the responsibility of the observer to keep them straight and to specify which is being employed. Furthermore, the number of different kinds of referents which a concept can include without losing its analytic usefulness is limited. If concepts become too flabby, description can proceed—and often does—without them.

The concept of power cannot be fully discussed here, but some questions about it should be raised. As a "systematic construct," this has been developed—so it is alleged—by the impelling clamor of the "real" world, by the accumulated insights of political scientists since Machiavelli, and by the need to rescue the field of international politics from the normative approach which characterized it in the 1930's. As noted, power emerges in these five books as a major category and concept. It is accepted by all the authors—either as a basic motivational assumption or as a general orientation to the field. Therefore, it is worth noting that whereas there is evidence of greater explicitness and of increased attention to definition, power is *not* subject to any rigorous analysis *as a concept.*

In the form in which it is stated, the power explanation of state motivation is a *postulate*—it is an *assumption* which *is taken*

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for granted rather than subjected to verification and testing. Once the struggle, urge, or drive for power is postulated as the basic motive for state action, it remains only to illustrate it, to discuss its forms and sources, and to inject it as the crucial variable in all relationships. Surely power does not have fewer terminological difficulties than "foreign policy," "national interest," or "nationalism"—all given detailed attention by the authors—yet as a term and as a concept, power is defined very briefly indeed. Padelford and Lincoln, and Strausz-Hupé and Possony, do not really define it at all. The latter refer to it as an urge or hunger.

Palmer and Perkins say: "Power of some kind is the *means* by which states implement their policies . . ." (p. 71); ". . . power is an essential element of politics . . ." (p. 72); ". . . '*power*' in a political context means 'the *power* of man over the minds and actions of other men' " (p. 74). These two authors use "power" in an inclusive sense to encompass *influence, force,* and *capacity* (p. 75). "To the *totality* of *a state's effectiveness* in world politics we apply the term 'power' " (p. 76). Schleicher's definition is: "Political power is the *ability to control and manipulate* the actions of others." "Since *all means of control are power,* physical power is only one of its forms" (p. 87). As noted above, Morgenthau defines power as "mutual relations of control," as *interest,* and as a psychological relationship in which the wielder exercises control over others through influence over their minds. What this boils down to is: power is control and means of control over others through influence on their thinking and action.

Difficulties with the power concept are fairly obvious. *Control, interest,* and *influence* are left unspecified, thus rendering the definitions cited nominal in nature. We do not know whether power refers to a symmetrical or asymmetrical relationship or both, whether control implies no freedom of choice for the party who is controlled, and whether control is limited just to some property or characteristic or behavior of the controller, or whether influence covers *any* change in the behavior of the influenced regardless of the source of change or the psychological mechanisms involved.

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Further ambiguities of a serious sort can be identified. As operationalized in the particular discussions, power seems to mean: a *quantity* of something (much of the elements of national power analysis falls in this category); a *capacity* which is somehow qualitative and not subject to precise measure though crude differentials can be discerned; a *relationship*, variously presented as ordinate and subordinate, superior and inferior, or balance of equals. In each case, it is a long jump from the variable to the behaviors which are of interest.

Very often the mere presence of a "power factor" is sufficient to constitute "proof" of its role in explaining action. Also the concept does not—as currently formulated—permit a fruitful, logical, and demonstrable link between power as quantity and power as relationship. "Why" questions are left unanswered. To say that state X desisted from a course of conduct because of state A's power does not, of course, adequately explain a complex situation in which non-power factors are presumably present, nor does it throw any light on *how* a "control relationship" actually operates or *why* one state *is* influenced by another. Even if one accepts the "power motive," one must still describe and explain the forms the resulting behavior takes—and there is no means of doing so by its usual definition.

As critics have persistently pointed out, the power concept is embarrassed by empirical events which it presumes to explain, but for which it cannot account. In part, this is a natural consequence which bedevils all single-cause or single-factor interpretive schemes. There is always something "left over." Therefore, the predictive efficiency of the concept is reduced materially. On the other hand, if the level of generality is such that too much ground is covered, then the risk is that, in explaining everything, the concept really explains nothing. In this case, either the concept needs more precise definition or a new concept is needed.

As sometimes employed, the power concept comes close to being meaningless. If power is control or influence, it becomes exceedingly difficult to find any social relationship or interaction which does not manifest it. If all participants in a social system are assumed to seek power, and if power is defined as all the means to any given end, one cannot imagine power being absent.

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Again, if the power factor or motive is postulated—instead of being held problematical by the observer—it is then a *constant*. Constants can hardly be cited as a "cause" of variables, i.e., of various behavioral consequences. Yet power slips easily from the status of assumption to the status of empirical phenomenon. Quite apart from this difficulty, it is a nice question whether a single-factor thesis should be permitted to carry on indefinitely as an assumption. Illustration is not proof. And usage is not necessarily a test of fruitfulness, especially when the concept is not critically re-evaluated. There is some evidence that power has developed from a hypothesis to a tested empirical proposition simply with the passage of time.

As one analyzes the empirical description which takes place under the headings of elements of national power, balance of power, the power struggle, forms of power, power strategies, and power problems, one is impressed by the fact that what is referred to by the term "power" has a significant "situational" aspect and is in fact often a resultant of several variables, not a variable in itself. When power as a motive or power as means is discussed, it must be *in general* without reference to particular states acting in response to particular stimuli in particular contexts. In complex situations focused on a problem or issue, it is the *combination* of conditions, motives, objects, relationships, and events which are perceived by the participants to be important and upon interpretations of which they act. Furthermore, the power equation—assuming this can be operationalized—would appear to be identifiable and measurable only after a situation has crystallized—in other words, after the fact. How good a predictor is the power factor? Actually, in the empirical materials treated by these authors, the situational and resultant qualities come through clearly, but the concept does not alert the user to these properties nor specify how they are to be handled analytically. The power concept is a static concept. As defined and employed, it is too ambiguous to be meaningful and too weak to perform the inclusive functions claimed for it.

Now it is *not* being suggested that the empirical phenomena so competently discussed under various power categories in these five books should not be given a central place in the study

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of international politics. Nor is it necessary to dismiss power as a totally useless concept. But it is legitimate to ask two questions: (1) Can the power concept be strengthened to the point where it can serve as a general theory in the most complete sense, as *the* basis of a frame of reference for the whole field, or should it be limited to a role as one subordinate concept in a system of concepts? (2) Can some of the functions attempted by the power concept be better performed by another concept or set of concepts?

IV

In addition to the foregoing conceptual difficulties, there are other evidences of weakness, omission, and neglect. Despite the unanimous recognition of the interconnections between domestic policy and foreign policy, the importance of public opinion, and the internal sources of national power, one has the feeling that the sociological foundations of state action have been relatively slighted. With some exceptions, there is no systematic outlining —to say nothing of detailed discussion—of the impact of total social organization and behavior patterns on foreign policy. Leaving aside the analysis of the material bases of national power, only some 130 pages out of the more than four thousand pages represented in these five works are devoted to the possible relationships between the nature of society, on the one hand, and its external behavior, on the other.

It may be answered that long discussions of nationalism and ideology, for example, do in fact constitute sufficient treatment of sociological factors. However, these two topics usually involve —in part, at least—the presentation of disembodied "forces" which "influence" policy. There is a determinant and a result. The level of abstraction and the formulation of the concepts are such that it is difficult to pin the analysis down. A leap from, say, national character to specific foreign policies is a long one indeed. There are several consequences of this "force-leading-to-policy" equation. First, it lends a determinate flavor to the analysis of state behavior, suggesting really no choice for policy-makers. Not only is reification of state and society invited, but the policy-makers may appear as mere puppets swept on by pressures and

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movements over which they have no control. This is especially true when societies or nations are endowed with "drives" and "spirits." Second, explanations of this order are lacking in depth. To say that nationalism causes or impels states to pursue their own self-interest does not reveal how this comes about. In other words, it does not help us to trace a sequential influence process from a behavioral datum "in society" to a behavioral datum at the policy-making level. Third, certain questions are not raised or are apparently not regarded as significant. What is the effect of a society's communication network on the views citizens have of the international environment? What values and value clusters are held in common by a majority of the society's members and how do such values affect policy choices? In what ways are the decision-makers dependent on the society and how does this affect policy-making behavior? That such questions remain unasked suggests that we do not have a set of criteria for specifying relevances in the social system and for analyzing social factors so that connecting links to policy-making can be established.

Earlier in this essay, the greater emphasis on policy-making as an analytic tool was welcomed. Despite marked progress, it is still true that no full-blown model of the policy-making process has been developed. In four of the five books, the foreign policy organization of the United States is discussed in detail. However, in each case, the analysis is primarily structural, dealing mostly with concrete agencies and their functions rather than with processes and relationships between them. In the absence of a frame of reference for analyzing policy-making as a process, it is difficult, if not impossible, to show how multiple factors enter into decisions and to provide a method for describing the simultaneous impact of multiple factors. The authors recognize that external (the world environment) and internal (the society) factors exert pressures and constitute limitations on the policy-makers, but a third set of factors arising from the immediate organizational setting of policy-making is either ignored or taken for granted. A good case can be made for the point that the failure to conceptualize adequately the policy-making process has added to the difficulties of tying in the sociological variables

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mentioned above. Even if we specify the social background, career patterns, and recruitment of policy-makers as relevant to an understanding of international politics, it is not possible to demonstrate the actual effect of these factors unless and until the process of policy-making is defined so as to permit translation of these factors into the determinants of policy choices. To repeat, while it is agreed that the study of international politics is concerned with real people—officials—who make choices and are subject to limitations on their desires, discussion often slips into the terminology of "forces," which seems to leave policy-makers in the role of robots.

Anyone who reads the footnotes of these five books must acknowledge the breadth and depth of learning indicated. The authors believe that international relations is an interdisciplinary subject and they all cite many sources outside of political science. Nonetheless, the extent to which the study of international politics has become a well-integrated subdivision of social science is open to question. Has maximum use been made of the possible contributions of all branches of learning? At first glance, it would seem that by far the most numerous citations are to history, demography, and economics, that chief reliance on other disciplines comes in connection with the elements of national power, and that it is almost exclusively the data rather than the theories of other social sciences which interest the authors. The bibliographies presented in these books are excellent, but the listings are not necessarily reflected in contributions to the authors' scheme of analysis.

It is hardly necessary for political scientists to be humble beggars at the doors of the other social sciences or to assume that fellow social scientists automatically "know more" than we do. Unfortunately, there have been conflicting tendencies both to overrate and to underrate what these outside enterprises can do for political science. But if—as these authors directly or indirectly admit—the study of international politics requires a wide variety of knowledge, then the question of whether all potential sources have been tapped is pertinent.

To import uncritically the concepts and theories from, say, sociology and social psychology is to listen to the call of the

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Sirens. On the other hand, to reject the newer developments in methods and techniques in other fields as "jargon" or "methodology," or as irrelevant because they do not deal with political phenomena *per se*, is foolhardy. If there are conceptual weaknesses which can be remedied, perhaps something is to be learned from those social scientists who are somewhat more systematic than the average political scientist is wont to be. It may be less important to borrow the particular concepts and categories from other fields than to learn about the most fruitful ways of constructing analytic apparatus.

Inasmuch as these textbooks are all recent, one wonders why certain interdisciplinary efforts were not at least mentioned. For example, there are the attempts to lay the foundations for a general theory of social action—aspects of which are clearly relevant to the attempt to put the analysis of international politics on a more general level. As another example, there is the focus of organizational decision-making—concepts of process and theories of choice. This work is being done by economists, sociologists, mathematicians, philosophers, and social psychologists. A third example is communications research. A number of empirical findings and modest theoretical advances in this sector would seem to have some bearing on the study not only of foreign policy techniques (persuasion, propaganda), but also of opinion formation within the state and non-governmental relations between societies. These examples barely scratch the surface. Before deciding that further interdisciplinary efforts are unnecessary, more penetrating questions should be asked.

V

The authors are fully aware of the difficulties which confront the development of more integrated and systematic procedures for political analysis. Though it is insisted that international relations is a separate discipline and though the advantages of separation are clear in many instances, it is also true that the field is still heavily dependent upon the stockpile of analytic tools available in political science.

Certain problems have plagued the formulation of powerful

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and fruitful concepts as well as the building of general theories. First, more dynamic concepts are needed to account for *process*. All the authors recognize change as a basic feature of international politics; yet, beyond description, the chief reliance for explanation is for the most part on state concepts. The latter can handle historical mutations over long time periods, but cannot handle shorter-run time periods plus changes, which are the essence of the political process. Second, the wide range and complexity of phenomena lead to multiple foci of analysis—institutions, behavior, organization, events, society, programs and policies, nation, group, and so on. To analyze such a complicated universe appears to require alternating perspectives and widely different kinds of categories and concepts. These perspectives and analytic devices must somehow be connected within a single frame of reference. The number and diversity of levels of generalization, of meanings, and of units of observation which can be accommodated in orderly fashion within a general scheme are limited. No general theory of international politics exists and there is no agreed frame of reference which embraces the phenomena or relates the major categories and concepts regarded by the authors collectively as important. A third problem—still unresolved—concerns the specification and identification of political phenomena. What is *political* and how is it to be identified? This is not a routine definitional exercise, since the decision on this question vitally affects the choice of conceptual schemes. Ambiguity on the nature of political phenomena may have delayed progress toward new categories and concepts.

Clearly, each of these works will stand on its own feet in terms of its purposes and internal logic. However, quite apart from the solid empirical contributions, perhaps the most significant implications of the various approaches set forth are the hints which can be inferred concerning some of the possible "next steps" in the search for greater order in this field. It may be suggested that the prevailing direction seems to be toward consolidation of generalizations on a higher level and toward a smaller number of related and coherent systems of analysis, linked by a more explicit and detailed definition of the field:

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(1) *Action analysis*—treatment of state action as a special case of social action in general, employing explicit analytic categories of *actor, means, ends,* and *situation.*

(2) Greater emphasis on *situational analysis* as an integrating device.

(3) *Decision-making* as an analytic focus, conceptualized to make possible an easier and more explicit handling of several sets of variables.

(4) More thorough and systematic description and interpretation of *non-governmental, intersocietal interactions* and their relationships to governmental interaction.

(5) *Categorization of relevant aspects or factors of any society* (social system) and exploration of relationships to non-governmental interaction and to policy-making.

(6) Establishment of well-defined state *interaction systems and processes,* properties of which are specified and typified in terms of such factors, forces, and conditions as objectives, strategies, communications, rules of conduct, institutionalization of policy-making, duration, interdependence of units, reciprocal expectations, and so on.

(7) Linkage of typical interaction systems and processes to *types of societies, types of decisional systems,* and *types of outcomes* (peace, war, etc.).

The ingredients for further integration seem to be present. While there is much yet to be done, we may be closer to another plateau of development than we think.

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THE SCIENCE OF POLITICS: New and Old

By ROBERT A. DAHL

David Easton, *The Political System: An Inquiry into the State of Political Science*, New York, Alfred A. Knopf, 1953, 320 pp. $4.00.
Eric Voegelin, *The New Science of Politics: An Introductory Essay*, Chicago, University of Chicago Press, 1952, 193 pp. $3.00.

POLITICAL scientists will soon need a vocabulary to describe, not, as the layman in his innocence might suppose, the world of politics with which they purport to deal, but rather the cleavages within the profession itself.

For the purposes of this review, it will be sufficient to distinguish the Political Scientist and the Political Theorist. The Political Scientist is often a theorist—in one sense, he is *always* a theorist—and frequently he believes strongly that an important requirement of theory is that it be explicit; therefore, he is not called a theorist. The Political Theorist, oddly enough, is often not a theorist at all but a historian, and insofar as theory is embedded in his history, it is more likely to be implicit than explicit; hence he is known as a Theorist. Between the Political Scientist and the Political Theorist, serious intellectual tension seems presently to exist. Inevitably, each caricatures the aims and methods of the other, and in time each even comes to believe in the caricature; straw men are set up and knocked down with great gnashing and thrashing; and the bones of old skeletons are rattled with such furious energy that one has the momentary illusion of life. It might all be good fun if the participants were not deadly serious.

Professor Easton (a Political Scientist) administers as thorough a drubbing to American political science as it is likely to get for some time. He attacks it because of its failures as a science. Professor Voegelin (a Political Theorist) argues in effect that it has been too much *a* science and insufficiently *the Science* of politics. It is clear, then, that among other disagreements the two authors conflict over the elementary meaning of words. Alas, much of the

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dispute is over words, but in the end there are also some basic but elusive differences in epistemological and metaphysical assumptions.

I

American political science, Easton observes, lags badly behind the other social sciences. Despite much misleading sound and fury, "political research has still to penetrate the hard core of political power in society" (p. 41). Moreover, political research over the last seventy-five years has neglected the phenomena of political change. Even the concepts used are primitive, vague, imprecise; terms like "dictatorship," "class," "sovereignty," "responsibility," "democracy," are replete with multiple evasive meanings. Generalizations exist, but they are vague and broad and command little agreement—or, if agreement exists, it depends upon ambiguity. These and other failures add up to a weak and laggard discipline incapable even of earning the respect of other social scientists.

Easton has some interesting and well-argued explanations for all this, and if his sleep is not disturbed at night by the noisy presence of the outraged shades of a very great number of deceased presidents of the American Political Science Association, I should be considerably surprised. A lack of concern with questions of methodology and technique; the traditionalist attitudes of the profession; its distrust of innovation in ideas; its excessively close tie with social policy—these are some of the causes. The results include an overemphasis on the mere collection of facts; a rejection of genuine empirical theory; premature attempts to prescribe public policies; excessive historicism; confusion and naïveté on the fact-value question; and an exponential decline in the intellectual rigor of political theory.

What then should be the focus of political inquiry? Easton examines several alternatives—the state, power, law, social groups, personalities, and motivations—and rejects them all. His own proposal is the outcome of roughly the following dichotomization. Objects of value to human beings must somehow be allocated. An allocation may be either authoritative ("when the people to whom it is intended to apply or who are affected by it

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consider that they must or ought to obey it"), or non-authoritative. Authoritative allocations, in turn, may be either societal ("considered authoritative for the whole society"—whatever this means) or non-societal (i.e., considered authoritative for—or by?—less than the "whole" society). Political science is (or, at any rate, ought to be) focused on the authoritative allocation of values for a society, or, more briefly, on the "authoritative policy of a society" (ch. V).

My summary is necessarily incomplete and there is a good deal more to the book than I have indicated, including some highly interesting observations on the idea of equilibrium in political analysis. Moreover, a summary does injustice to the tone of the book, which is modest, thoughtful, and judicious. Easton has had to do some simplifying, but he is nearly always aware of the complexities that his argument may overlook in its attempt to sketch out boldly—and courageously—a fair diagnosis and a useful prescription.

There are, however, some difficulties that Easton never fully grapples with and the most important ones stem from the central question that concerns him, namely, "What is political science?" Now this is a question of importance to departmental chairmen, deans and perhaps even philanthropic foundations, but as a scientific question it is trivial. It is on the analogue of the natural sciences that Easton relies for his methodological commitments and criticisms, and yet, so far as I am aware, the history of the natural sciences does not reveal that the great innovations in theory ever came about as a result of questions of this kind. "What is mechanics?", "What is biology?", "What is physics?"—these are not the questions that have caught the attention of a Newton, a Darwin, or an Einstein. The query, "What is political science?" is curiously metaphysical, a search for "essences," and the pursuit of an answer cannot by itself provide a single scientific theorem. For in the end we have no more than a definition of political science and a proposal that the definition be employed. But we cannot know whether empirical theory will be facilitated by this definition, or by an alternative to it, until the empirical theory is actually produced. And empirical theory can be produced only by constructing and testing propositions about the real

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world, not by defining political science. Hence the question cannot long be pursued without degenerating into a sterile definitional process.

It would be unfair to say that Easton is unaware of these objections; but his preoccupation with this question does get him into some difficulties, of which three are important. The first, oddly enough, is a simple normative ambiguity of a kind that Easton lucidly examines in the approaches of other writers. We are told that political science *is* the study of the authoritative allocation of values for a society. Yet much of the book demonstrates that this is precisely what, at present, political science is *not*. One concludes that Easton's "is" is really an "ought." But why *ought* political scientists to concern themselves with authoritative social policies? Presumably because by doing so they will be able to construct a body of useful, coherent, and unifying empirical theory about certain aspects of social action. But how do we know that this prediction will be fulfilled? Well, . . . there's the rub. Obviously, we have little way of knowing in advance; indeed, we cannot even draw upon any analogies for a reasoned guess as to the probable outcome of this particular focus of inquiry. Easton can vindicate his hunch by providing us with a body of empirical theory but, so far as I am aware, by no other means. In this sense, then, the book is bound to be anticlimactic.

Second, the search for an essence encourages Easton to draw false dichotomies, for once he has fixed upon the authoritative allocation of values in society as *the* essence, other approaches to theory evidently cannot be political science. For example, Easton rejects "power" as a focus for political science by the argument that "power is only one of the significant variables. . . . Political life does not consist exclusively of *a struggle for control*" (p. 117; italics added). But surely this is a confusion of two quite separate propositions. One may well argue that power (or control) relationships are potentially of such theoretical importance that political scientists should focus their attention exclusively on these relationships. In this sense, one would define political science as the analysis of power relations.[1] However, the proposi-

[1] To urge this focus would, of course, be to commit the same fallacy of "essences" mentioned above, but we are only concerned with Easton's criticism here.

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tion just enunciated is entirely different from one defining political science as the analysis of the *struggle for* control. The advocate of the first position might well say that the analysis of power *relations* is far broader, and would include struggles *to be* controlled as well as efforts to achieve a vast range of human goals—only so long as a control relationship of some kind exists.

The third difficulty is Easton's failure to provide us with anything like a set of "operational" or "observational" definitions of his key terms. He is entirely right, I think, in arguing that the striving for rigorously operational definitions may kill a science at infancy. Newton, after all, succeeded with the idea of "force" which, as Mach pointed out at the end of the nineteenth century after two hundred years of productive utilization of the concept, was sometimes little more than an elaborate circularity. Nonetheless, Easton's proposals are so ambiguous that if he had examined his own definitions as rigorously as he does those of his predecessors, I believe he might either have refined them or abandoned them entirely. Thus, how do we distinguish an "authoritative" allocation? Easton answers, "When the feeling prevails that it must or ought to be obeyed." But by whom and by how many? Criminals evidently do not believe that criminal statutes must be obeyed; but if a law against murder is not an authoritative allocation, what is? And then there is "societal." When is an allocation considered authoritative for the "whole" society? Who considers it so? "Whole" meaning every last man, woman, and child? Does Easton mean, as he sometimes writes, that the allocation is "societal" if it is considered authoritative *by* all? Surely this is not always the same as authoritative *for* all?

These queries are not mere cavils. They take us, I think, back to the main difficulty. If Easton had tried to use his concepts in constructing a body of theory, he would soon have discovered the extent to which they are useful; he might have, and doubtless would have, modified or abandoned them and developed others. And, of course, that is the point: we really have no evidence that the categories he proposes are useful and workable.

Fortunately, Easton's critical and analytical chapters—his careful examination of the state of political science, how it got that way, and some of the methodological and conceptual problems it

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now faces—make rewarding reading. If he has centered his inquiry around the wrong question, as I see it, his good sense and courageous skepticism have produced a book more significant than the question it poses.

II

We need not go beyond the opening page of Professor Voegelin's book to realize that we are going to encounter a vastly different argument. Voegelin and Easton agree on scarcely more than two points: that there have been too many political scientists collecting too many facts without a framework of theory, and that political science is in a parlous state. Voegelin's argument is much more difficult to summarize than Easton's; no doubt this is partly because it is written in an extraordinarily awkward, polysyllabic language only distantly related to the one normally used in written discourse in English-speaking countries (Voegelin is fond of sentences like these: "The problem of an eidos in history, hence, arises only when Christian transcendental fulfilment becomes immanentized. Such an immanentist hypostasis of the eschaton, however, is a theoretical fallacy"—p. 120); and partly because of his offhand unconcern with definitions, which he will sometimes fling out (as if casting a penny to a beggar) and then, like as not, ignore as he uses his private lexicon in which the same word has a number of different and often indeterminate meanings. Hence this is not only an irritatingly contrived book but one that does a disservice to his argument.

We have here, indeed, three books in one. One is a polemic against positivism; the second is a historical examination of the rise of what Voegelin calls "Gnosticism"; the third is a demonstration—a kind of intellectual tour de force, really—of a methodology which purports to be truly Scientific in a way that lowercase science is not.

The polemic against positivism is the weakest part of the book and, like many introductions, it was perhaps written after the main chapters—originally six lectures—were prepared. It begins with a curious definition of science—we had better write "Science"—as "a search for truth concerning the nature of the various

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realms of being." If we can have "un-facts," we can equally well have "undefinitions," and Voegelin may be said to have undefined science as most of us understand it. However, he must be conceded his freedom to choose his own definitions; the modern right to redefine science dates back at least to Mary Baker Eddy. But more than words are at stake. For the central question is: What are the canons of proof and disproof in Voegelin's Science? How do they differ from the canons of science? Unfortunately, nowhere does he provide us with an answer to these questions.

He comes closest to suggesting the general direction of an answer in one of his criticisms of positivists, namely, their view that scientific propositions differ in kind from propositions asserting a value. In recent years, this question has been knocked around so unmercifully in a number of solemn meetings of political scientists—rarely have so many done so little with so much—that I shall drop the point in a moment. But Voegelin's *denial* of the positivist assertion presumes a counterproposition that he never provides. Does he really mean to say that empirical propositions are *not* distinguishable from value propositions? Does he mean that one validates both by the *same* methods? If so, what are these methods? Unfortunately, the book is silent on these crucial points.

Voegelin blames positivism for the evils, as he sees them, of contemporary social science. But insofar as he asserts that positivism *causes* certain results—for example, an excessive accumulation of facts and inadequate attention to the role of theory—his analysis is no more than a crude *post hoc, ergo propter hoc.* And insofar as he attacks positivism for *preferring* these results, I believe he misinterprets it; for modern positivistic philosophers of science would be among the first to agree on the central role of theory in science and on the triviality of fact collection without theoretical orientation.[2]

Indeed, Voegelin does not meet the *modern* positivists—he simply ignores them. This is all the more curious since he must have been in Vienna at the time the Vienna Circle was most active; it is difficult to believe that a writer of such massive erudi-

[2] E.g., Carl Hempel, *Fundamentals of Concept Formation in Empirical Science*, Chicago, Ill., 1952.

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tion, who uses a third-century document with more skill than most of us use the *New York Times,* was ignorant of the theories developed by his colleagues and remains so to the present day. Yet it simply will not suffice to rebut Comte, who is nearly a century dead and sometimes was a bit of a fool alive, and assume that one has come to grips with positivistic theory. Max Weber, of course, is a worthier opponent, but I believe he is charged here with crimes he did not commit, such as that "he treated all 'values' as equal" (p. 20). It is time that someone of Voegelin's stature should close with the positivists; but in this volume he simply runs past them brandishing a sword, and even when he manages to behead the village idiot on the way, his performance, although exotic, is not spectacular.

Much the most solid and challenging part of his analysis is the historical treatment of "Gnosticism." There is not space here for more than the briefest summary, and even so it will be necessary to ignore one strand of his thought—concerning "representation" —that is interwoven with the examination of Gnosticism.

Pre-Christian societies were regarded as representatives of a transcendent truth. (Voegelin reifies endlessly and I shall follow suit.) Christianity, particularly at the hands of Augustine, "radically de-divinized" political society; for "the spiritual destiny of man in the Christian sense cannot be represented on earth by the power organization of a political society; it can be represented only by the church" (p. 106). But this happy medieval solution was destroyed by modern Gnosticism, with terrifying results.

It is in keeping with the murkiness of concepts in the book that Voegelin never tells us with any precision what he means by modern Gnosticism. Whatever may be the analogies with gnosticism in the conventional sense of a certain historic body of doctrines in rivalry with early Christianity, we are never told, "for the gnosis of antiquity . . . [and] the history of its transmission into the 'Western Middle Ages' " are brushed aside for lack of space (p. 124). Sometimes by Gnosticism Voegelin seems to mean doctrines that assume the possibility of forecasting "the course of history"; sometimes only those doctrines that forecast the attainment of human perfection or salvation here below (p. 120—but cf. p. 126). In any case, Gnosticism "re-divinizes" society

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by abolishing the Augustinian distinction between (1) a transcendent truth and spiritual salvation attainable through the church, and (2) the profane history of ordinary society which has no eschatological meaning, no direction toward fulfillment, but is a mere "waiting for the end."

The "first clear and comprehensive expression" of Gnosticism is found in Joachim of Flora, but its later expressions include Puritanism, liberalism, progressivism, and totalitarianism; among its great prophets are Calvin, Hobbes, Marx, and of course Comte. "Totalitarianism, defined as the existential rule of Gnostic activities, is the end form of progressive civilization" (p. 132). In sum, "the immanentization of the Christian eschaton made it possible to endow society in its natural existence with a meaning which Christianity denied to it. And the totalitarianism of our time must be understood as journey's end of the Gnostic search for a civil theology" (p. 163).

However, "human nature does not change [*sic*]. The closure of the soul in modern gnosticism . . . cannot remove the soul and its transcendence from the structure of reality." It is therefore a "reasonable assumption that the reaction against gnosticism will be as world-wide as its expansion" (p. 165). And ". . . It is at least probable that the date is less distant than one would assume under the impression of Gnostic power of the moment" (p. 166).

Obviously, it would be impossible to assess the validity of Voegelin's thesis in brief compass. Much more interesting for our purposes, however, is the question: How *could* we go about appraising it if we set out to do so? To employ a term that Voegelin no doubt loathes, is there any sense at all in which the theory is "testable"? Here we come to the third of the books which the author has combined under one title: namely, a demonstration of the methodology of the New Science.

What is this methodology? We get hints, but hardly more. Thus we are told that the analysis uses "the Aristotelian method of examining language symbols as they occur in political reality, in the hope that the procedure of clarification would lead to critically tenable concepts" (p. 52). Now one can scarcely object even in lower-case science to a preliminary examination of the customary usage of a set of terms, in order to single out some

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terms for refinement. But what is a "critically tenable concept"? In ordinary science, "concepts" are propositions about definitions, classes, rules for measurement or comparison, and "causal," "functional," or other relationships among phenomena. Each type of proposition is "tenable" according to rather different criteria. Are the criteria of the New Science different from those of the old? If so, what are they? If not, then what—in the hallowed phrase—is all the shooting about?

Almost any page we read suggests the same questions. Does Voegelin have a concept of "causation" or functional dependence different from that of ordinary science? Although it is extremely difficult to tie him down, he appears to be asserting that "Gnosticism" as an "idea" or intellectual way of interpreting society is a "cause" of totalitarianism; and he seems to be predicting that the destruction of Gnosticism is necessary to a destruction of totalitarianism. (*Inter alia*, if liberalism is Gnosticism, it seems to follow that liberalism must be destroyed also.) Without commenting specifically on these cosmic generalizations and predictions, it is surely relevant to ask: are the tests of "causality" or functional dependence different in the New Science from what they are in the old? If so, just how are we supposed to test Voegelin's propositions? No doubt this is a vulgar, old-science type of question, but if I don't ask it, somebody equally disagreeable is sure to do so.

One final example. One of the most curious sections of the book is a short detour on historicism.[3] Voegelin is proving that "history" cannot be predicted, and his argument runs: "Things are not things, nor do they have essences, by arbitrary declaration. The course of history as a whole is no object of experience [so far so good—R.A.D.]; history has no eidos, *because the course of history extends into the unknown future*" (p. 120; italics added). Thus Voegelin has shown, as he thinks, a fallacy of historicism so elemental that one is hard put to "explain seven centuries of intellectual history" during which it was ignored. But look closely at his argument. He has simply postulated the conclusion he set

[3] I am not defending historicism or, for that matter, positivism. The question is as to the validity of Voegelin's New Science, not the validity of the views he attacks. One may logically conclude that both historicism and Voegelin's attack on it are false.

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out to prove. If the future is really "unknown," then by definition it cannot be predicted. *But that is the issue.* And if the future is really "unknown," then ordinary science is also based on a fallacy—the same fallacy; for it assumes that, given certain kinds of knowledge about past events, future events can be predicted. Now if Voegelin's New Science is based upon the assumption that future events can never be successfully predicted from past events, then it is a very revolutionary science indeed. Indeed, he has not only "un-defined" science; he has un-sciened it.

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RESEARCH IN MILITARY AFFAIRS

By ROGER HILSMAN

Sir John Slessor, *Strategy for the West*, New York, William Morrow and Company, 1954, 180 pp. $3.00.

Raymond L. Garthoff, *Soviet Military Doctrine*, Glencoe, Ill., The Free Press, 1953, 587 pp. $7.50.

Civil-Military Relations: An Annotated Bibliography, 1940-1952, Prepared Under the Direction of the Committee on Civil-Military Relations Research of the Social Science Research Council, New York, Columbia University Press, 1954, 140 pp. $2.00.

C. N. Barclay, *The New Warfare*, New York, Philosophical Library, 1954, 65 pp. $2.75.

SOCIAL scientists long ago accepted the proposition that military affairs are a subject with which they should concern themselves; but recently, in response to the increasing role that force or the threat of force plays in foreign affairs today, interest in the field seems to have quickened considerably. The establishment of a Committee on Civil-Military Relations Research by the Social Science Research Council is a case in point. Alerted by this heightened interest in research on military affairs, one is encouraged to consider each new publication in the field not only as a contribution to our knowledge, but also as a *type* of contribution—as a stimulus to speculations about which problems in the area a social scientist should work on and what he can expect to accomplish if he does.

I

In the social scientist's file of military problems, the fattest folder of all would very likely be one containing problems of adapting civilian activities to military needs. These would include such matters as managing a war economy so as to maximize the production of weapons, allocating available manpower to suit the needs of both industry and the armed services, and organizing the complex land and sea transportation system needed to carry soldiers to the place where they are to fight and to keep them supplied with food, equipment, and ammunition while they are there. And there is certainly nothing inappropriate in this folder

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being fatter than the rest: in war, it is only the power at the end of this long chain, the soldiers and weapons on the battlefield, that counts, and any link in the chain is just as vital as any other. During World War II, for example, the bureaucratic techniques of keeping track of from ten to twenty million men and of taking perhaps as much as half the national income away from the citizenry and channeling it into war production did as much to disarrange the neat traditions of strategic doctrine as either the tank or the airplane.

In another folder would be the problems arising from the use of military force or the threat of force as an instrument of foreign policy—when and where to use or threaten to use force, how much to use, what demands on foreign policy will develop in a given set of circumstances during the process of implementing decisions to use force, what subsidiary effects the use of force has in those circumstances, and the relation of these to the primary goals. In any rational ordering of foreign affairs, decisions to use force or decisions about the circumstances in which it will be used in the future—decisions which imply preparations of special kinds at specific places—should be made with the whole range of goals, costs, subsidiary effects, and alternatives in view.

From a "military" point of view the JCS decided, as we all know, that South Korea should not be defended, that it had no "military" value to the West, and that in fighting there the West would be under a handicap. All this was probably true; yet when the attack came, the decision was to fight. The JCS apparently made their decision on what they regarded as solely "military" grounds, by assuming that the only political goal was the military security of the United States and that the only means involved was military force. The problem to them was thus a technical military one which left out all other considerations, including the effect of abandoning South Korea on future Communist behavior and on our allies.

But the absurdity of letting technical military considerations dictate the decisions of foreign policy should not obscure the fact that the military often have perfectly legitimate grounds for making demands on foreign policy. The most obvious of these are the need for allies, in general and at specific places, and the

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need for lines of supply and bases. One example is the complete change in our attitude toward Spain. Another example, but of a different and more subtle kind, is the effect of mobilization plans in World War I. In 1914, mobilization plans were so elaborate, requiring each movement to interlock precisely with every other, that once mobilization was started war had to follow: any attempt to halt the process would have left the nation in defenseless and chaotic confusion. All this seems rather quaint and old-fashioned today, but what was true of mobilization plans in the horseless-carriage days may be equally true of the Strategic Air Command in an age of jets. Or, which may be even more ominous, there will undoubtedly be some very high-ranking advisers who are convinced that SAC should not wait and who will argue their case vehemently and persuasively.

These examples illustrate that decisions about the use of force in foreign affairs must take into account the demands to which technical military considerations might give rise. But the existence of such a thing as military logic should in no way alter the basis for decisions about either the use of force or the way it is used. The criterion remains that of effectiveness in achieving political goals, and judgments based on this criterion, of course, require consideration of much more than military technicalities. And this may apply to decisions on the field of battle as much as it does to grander strategy.

II

Granting both that the criterion for policy alternatives, including alternative military strategies, is effectiveness in achieving political goals and that foreign policy must take into account the demands stemming from technical military considerations, one is not surprised to find that the books by Sir John Slessor, a Marshal of the Royal Air Force, and Brigadier C. N. Barclay, editor of the British *Army Quarterly*, are as much concerned with the problems involved in using force to achieve political goals as they are with technical military problems. The burden of Barclay's book is that since there is no hope at all for a settlement of the differences between East and West and since both

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recognize that an all-out war would be ruinous, cold war will be with us for a long time and the West should recognize this and set about acquiring skill in its techniques. He then proceeds—in what is, unfortunately, unoriginal and shallow detail—to discuss the potentialities of these techniques: propaganda, underground warfare and sabotage, limited war by proxy, the armed threat, and political obstructionism.

Slessor's book, however, is somewhat more suggestive. He begins by saying that the object of Western strategy should be to drive militant Communism back behind the frontiers of Russia and China (which Slessor regards as the Soviet's partner rather than satellite). But not by force: Slessor feels that in the face of a sound and consistent Western policy, the Soviets might come to the idea that a belt of neutral states which would relieve them of their present burden of holding down several million resentful people would provide an acceptable measure of security. Slessor is convinced, in fact, that the West has in its hands an inoculation against a recurrence of most of Europe's old rivalries and even against war itself. The vaccine, of course, is the atomic and thermonuclear bomb.

Slessor argues that since there is absolutely no chance of anyone winning an atomic war, no nation will resort to war as an instrument of policy if its leaders are convinced that the war will be atomic. Thus, by being determined to use atomic weapons from the outset and by making this determination clear beyond the possibility of misunderstanding, the West can actually abolish war.

Moreover, it is only by abolishing war altogether, Slessor feels, that the West can defend itself at all. He believes that fighting a war with a broken back, as the British describe the situation of a country that has been the victim of nuclear bombardment, is impossible. Presumably on the basis of technical military considerations, he also feels that it is equally impossible to limit a big war by excluding nuclear weapons. "Using the nuclear weapon" and "going to war," in the sense of total world war, are, he insists, synonymous. Furthermore, defense against nuclear weapons would be a complete failure if it let only one bomber through, and even though a 100 per cent kill-rate against bombers

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may prove to be scientifically possible, it will never, he argues, be economically so. It would cost 25 billion dollars to build a defense system against manned bombers, and Slessor asserts that this sum on top of the costs of the other forces needed would bankrupt even the American economy, not to mention the economies of the other members of the Western coalition. Then, not long after the system was completed, the long-range missile, traveling at many times the speed of sound, would replace the manned bomber; and an entirely different and even more expensive system would be needed. The West, consequently, has no choice in Slessor's view but to rely on the Great Deterrent.

But for all his faith in the atomic weapon as a deterrent, Slessor seems to recognize that the Soviets might prefer the chance of victory in war, however slim, to certain domination, and that as a minimum his strategy requires a complementary political and diplomatic context. Not only does he set the political goal at something less than eradicating Communism, pointing out that fear may also be a motivating factor in Soviet behavior, but he also argues the benefits of negotiation, the virtues of secret over open diplomacy, the true nature of appeasement, the importance of recognizing Communist China, and the desirability of a certain amount of East-West trade.

He also recognizes that there might still be small wars; and, dodging completely the question of how to distinguish one aggression from another, insists that the Sunday Punch, as American airmen call the atomic strike on the Soviet homeland, be reserved for the big war. If there are still to be small wars, conventional forces will also be needed; but, again concerned with the danger of national bankruptcy, Slessor argues that these should not be large. He feels that, in any event, nuclear weapons obviate the need for large conventional forces, and that the West is consequently free to concentrate its resources on the Deterrent.

The questions to which Slessor addresses himself—the possibilities of liberation without the use of force, the extent of China's independence, the level of destruction which the Soviets would find prohibitive, the deterrent effect of threatening to atom-bomb Soviet cities when the Soviets can also atom-bomb ours, the point at which defense expenditure would "bankrupt"

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the American economy, the technical feasibility of limited war, the changing limitations and possibilities posed by the new weapons, and the results to be expected from alternative strategies—these questions seem to be typical of the kinds of problems that will arise in the development of any foreign policy in which force or the threat of force is a component. They are also questions on which something can probably be said on both sides and by people with a wide variety of skills and knowledge, including those of the social sciences. Economists, for example, would certainly have something to say about what constitutes national bankruptcy, as would other social scientists about such matters as the circumstances in which it would be politically possible to use the Sunday Punch when the Soviets have a Sunday Punch of equal power, the kinds of changes in the Soviet Union that would be necessary to permit liberation without the use of force, and the other factors in addition to level of destruction on which the success of a policy aimed at deterring the Soviets might turn.

III

If the social sciences have something to offer in elucidating the place of war in national policy, the more research effort put into this area the better. Since the opportunities for steering research one way or another are not great, every chance should be made the most of, and a bibliography permits at least a little gentle nudging. In the very act of choosing a title, such as *Civil-Military Relations*, one portion of a field is cut out from the rest. Within the sub-area chosen, furthermore, the makers of a bibliography must still select and reject, usually doing so by means of the different categories by which the materials are organized. Inevitably and deliberately, one aspect of a field is emphasized and others are obscured. A bibliography, then, is in one sense a contribution to the subject of where research should head for as well as a description of where it has been.

In the bibliography under scrutiny, the material selected for inclusion is divided into five parts. Of these, Part I deals with national defense and the American governmental system and includes general studies of ideas about civil-military relations,

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histories of the development of these relations in the United States, discussions of the relationship between Congress and the military establishment, and analyses of legal and constitutional questions. Part II is concerned with organization for war and defense and with military management. Centering on civil-military relations within the executive branch of the government, it includes material on unification of the armed services, the organization and work of the National Security Council, control of atomic energy, and administration of the different services. Part III is concerned with national security and foreign policy and includes chapters on "Defense and Diplomacy," "Military Government and Occupation," and "Intelligence and Psychological Warfare." The chapter on defense and diplomacy, which is described as concentrating on coalition warfare and civil-military relations at the international level, lists most of the more famous war memoirs, histories of Allied relations during the war, and descriptions of NATO and the Mutual Defense Assistance Program. Part IV covers manpower and logistics, and Part V—"The Growth of the Military Establishment and Civilian Society"—such varied topics as civilian defense, science and technology, information and public opinion, and certain aspects of internal security and local administration.

All these are important problems, well-deserving further research, and the bibliography in question, which is an excellent and useful catalogue of the work in this field, serves an important purpose by directing attention to them. But there are dangers here, and W. T. R. Fox, the author of the preface, has wisely stepped in with a warning that makes the danger clear. In his opening paragraphs he says that the phrase "civil-military relations" has connotations which make the Committee wish a better term were available to describe its range of interest. He adds that public concern today is directed not so much at preventing the armed services from becoming either pawns or prime movers in domestic politics as it is at energizing our cumbersome defense machinery, enabling the services to formulate a military policy calculated to support the country's foreign policy, and developing the civilian capacities necessary if political and military means are to be effectively reconciled.

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Fox's point is that the problems which are the subject of relations between civilians and soldiers are more important than the problem of maintaining civilian supremacy. In the United States there are already any number of barriers to a political military—the country's long history of anti-militarism, the recruitment systems which create a corps of officers nurtured on those feelings, the officer rotation system, and especially the long-standing tradition, which is tightly woven into the whole fabric of American military custom, that the officer corps should be apolitical. The military in the United States, after all, take off their uniforms and put on civilian clothes the moment they are off duty and do not hesitate to become shopkeepers and businessmen as soon as they retire—behavior at which Europeans, for example, never cease to marvel. Now and then a military hero may become President, but the Army as a whole has never pushed him there or followed him into politics when he arrived.

Given these barriers, research in the field of civil-military relations should focus, as Fox suggests, not only on the people who are making decisions and the relations between them, but on the substance of the problems they face—the problems of how to develop our military strength, when and where to apply it, and how to ensure that it furthers rather than obstructs our political ends. A study of the behavior of the people who make decisions about these problems is an essential element in understanding the development of foreign and security policy and in an attempt to improve that policy. However, to be meaningful, a study of behavior must be made not only within the context of the problems to which the behavior is related, but also as a part of a larger study aimed at increasing understanding of the factors shaping those problems.

IV

If the first folder in a file on military affairs contains problems concerned with adapting essentially civilian knowledge and techniques to military needs, and the second problems arising in the use of force to achieve political objectives, the third folder would contain problems of weighing one military consideration against another.

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It is in this area of technical military problems that Garthoff's book falls. Armed with the obviously acceptable proposition that a nation must publish in manuals and military periodicals the doctrines it uses to train its armies, Garthoff has sifted through a warehouseful of material, shelf by shelf and corridor by corridor. In Part I of his book he traces the development of present doctrine, concluding that its sources lie more with the Imperial Russian Army and with Western military thought, especially that of Clausewitz, than with Marxism-Leninism. In Part II, he spells out the doctrines themselves, pinning his data on the framework provided by the traditional Western military concept of principles of war. In Part III, he describes the doctrines that are peculiar to each of the three services—land, sea, and air.

In studying a complex body of material such as Soviet military doctrine, each possible approach has both advantages and disadvantages. Probably the greatest disadvantage of the one used here, the classic Western conceptual framework of principles of war, is that it chokes off any attempt to use this entirely different body of military doctrine as a springboard for new theoretical insights into strategy itself.[1] Also, the formal, orthodox sources and methods associated with this framework tend to emphasize written materials and to slur over the evidence provided by behavior in battle.

Recognizing these inherent disadvantages, Garthoff has taken pains to provide as much as he can of the needed contrast between doctrine and practice by using German sources with skill and effect and by including in an appendix a brief analysis of the campaign immediately following the initial German attack in 1941.[2] But even if he had not taken these measures, Garthoff would undoubtedly still have been right in choosing the approach he did for a work which is in many ways a pioneer. With the concept of principles of war as an organizing framework, both the similarities of Soviet and Western doctrine—as on defense—

[1] The inadequacies of the principles-of-war framework, which tries to reduce a body of theory to a number of common-sense propositions, has been pointed out by Bernard Brodie, "Strategy as a Science," *World Politics*, I, No. 4 (July 1949), p. 467.

[2] As Garthoff points out in this analysis, there were in this campaign severe departures from the doctrines. Completely ignoring their own teachings, the Soviets in 1941 strung out their forces along the border, making almost no attempt at a defense in depth and failing to take even the elementary precaution of providing a mobile reserve.

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and the differences—as on mass and the concentration of both infantry and tanks—stand out, sharp and clear. This focus on similarities and differences convinces the reader that Soviet doctrine is essentially sound and disabuses anyone who still has hopes that a rigid and fanatical adherence to false doctrines will bring the Soviet Union to catastrophe. In general, when the Soviets differ from Western doctrines, they are merely making the most of their peculiar circumstances, as their doctrines on mass merely utilize their advantage in manpower. At other times, when their doctrines are actually faulty, the Soviets are far from rigid. As Garthoff says, as soon as they realized the disadvantages of retreating in long, tiring moves, they did not hesitate to adopt their enemy's tactic of leapfrogging in short jumps under cover of specially trained rear guards.

A final advantage of a study of this kind is that, since it catalogues the whole range of Soviet doctrine, it can serve as a referent for assessing—with a time lag—changes in Soviet thinking. The Soviet General Staff, like the Western ones, must sooner or later reexamine everything they have ever thought about war in the light of conditions posed by the atom. At the time Garthoff was doing his research (1952), there was very little available on how they were going about this reexamination. But since any changes in doctrine that would require extensive changes in organization or training could hardly be kept secret, this paucity of data was probably not so much a tribute to the Soviet system of internal security as it was an indication that the Soviet General Staff, like the Western ones, had not yet begun to think seriously about the problem. The recent appearance of articles on the subject in *Red Star* indicates that they have finally set to work and Garthoff's book will be a valuable base line from which to measure their progress.

V

If Garthoff's book symbolizes one kind of effort required in this area of what we have loosely called technical military problems, the reexamination required by atomic weapons symbolizes another—the problem of recognizing the battlefield implications

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of new techniques and inventions as opposed to the problem discussed earlier of adapting essentially civilian activities to a military context. The surge of railroad building in the nineteenth century, for example, brought on just this kind of rethinking. By permitting a very rapid mobilization and concentration of much larger armies and a quick shifting of troops from front to front, railroads gave the nation whose staff officers first understood their strategic potentialities a power on the field of battle several times as great as that of less foresighted enemies of equal size.

And in this purely military part of strategy as much as in any of the others, foresightedness has been the hinge on which national survival has turned. Take as an example the Battle of Britain. Quite apart from all other considerations, an invasion like the one at Normandy could never have been launched without a base of the size and nearness of England. Yet after the losses at Dunkirk, as we now know, there was not nearly enough strength in Britain to meet the invasion Hitler would surely have launched if he had believed that the RAF was helpless to intervene. Along with Stalingrad, the Battle of Britain stands as a time in which the outcome of the whole war hung in the balance.

The balance was as delicate as it was perilous. It was not until after Munich that Britain gave top priority to producing fighter planes, and at first the fighting in France and then Dunkirk drained off precious strength. The shield was very thin—at one point there was in all Britain only one squadron not committed—but in the end it held.

The credit for giving Britain the weapons to win this momentous battle apparently belongs to a small group of unusual men who, in a day when the offensive and bombers were shibboleths, were able to visualize the strategic pattern of the coming war, sort out the significant elements, and see that initially everything would depend on the Fighter Command and the air-warning network serving it.[3] It was through their efforts that decisions were made in 1934 and 1935 to order unusually fast fighters with the then extraordinary armament of eight machine guns and to

[3] Chester Wilmot, *The Struggle for Europe*, New York, 1952, pp. 33-55.

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support research on what was to be radar. It was also through their efforts that the final decision was made in 1939 to go ahead on fighter production.

Notice that these decisions required an acute understanding of the nature of future war and of the place of air power in it, not only as a strategic weapon attacking a productive base, but as a tactical weapon, especially in defense against a large-scale waterborne invasion. Notice also that it was not so much the battle experience of World War I, Ethiopia, or Spain that brought forth this understanding as it was an act of intellectual effort, growing out of insights into the nature of strategy and developing technological conditions that permitted the relevant experience to be separated from the irrelevant.

VI

In some periods of history there may have been little need for people with an intellectual orientation toward the profession of arms or for the results of their work and discussion, but it is impossible to believe that this is true today. Nuclear weapons may well make the next war, if it comes, the very last one. On the other hand, we may find that it is still possible to fight something that would be recognizable as war; and if it is, we must be certain that we are prepared for that possibility, at least so long as we treasure the nation either as a form of social organization or as the best available guarantee for other values.

In an age of H-bombs, the opportunities for a trial and error approach in either warfare or a foreign policy in which force is a demanding as well as a serving component are almost nil. The importance of both civilians and soldiers doing their homework is therefore that much greater. Unfortunately, however, there is little of the necessary kind of intellectual activity within the American military services themselves. Although the various service journals try hard to stimulate the kind of debate that the scientific professions use in shoving back the boundaries of knowledge and understanding, there are a number of fortified positions standing in the way which make the going tough. One of these is a tradition in the services against professional writing of any kind

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except memoirs: military men are doers, not thinkers, and even Mahan was promoted to admiral only on retirement and then only because all captains who had served honorably a certain number of years in grade were automatically given this pat on the back when they finally departed. Another, of course, is the anti-intellectualism that seems to be growing today; a third is the natural outlook of men whose main work is conducting operations. Familiar with the virtues of energy, determination, and loyalty to the policy to be implemented, but not with those of subtlety, qualification, and reserved judgment, the operator looks on intellectual brilliance, which questions assumptions that are felt to be beyond question, with a doubt that is probably tinged with fear. Not that the services do not have bright and able young men; if anything, they have more than their fair share. There are also programs to educate these men, including graduate training in the universities. But the alert and ambitious quickly realize that the only kind of brilliance that pays off is one that operates within the accepted set of assumptions, manifesting itself by reaching the same conclusion the commanding general would, but several seconds sooner.

There are, of course, intelligent generals, highly intelligent ones; but intelligence is not the same as intellectuality. There are even one or two intellectual generals, but they are generals partly because ambition, itself entirely commendable, has led them to muzzle their intellectuality at least in their professional field. For in the military, intellectuality is a talent like singing or piano playing: nice for parties, but suspect in more serious business. As the Old Man said to the young Melville Goodwin when he warned him against sticking his neck out to support a "singing" officer, no matter how capable he appeared to be, "When you're choosing personnel, select a good sound poker player or a golfer or someone who likes fishing, and you know where you are, because those types have stability. Put the prima donnas in Intelligence but keep them out of Operations."[4]

If the animosity and intransigence of the Soviet Union continue, more and more of foreign policy is likely to be concerned

[4] John P. Marquand, *Melville Goodwin, U.S.A.*, Boston, 1951, p. 346.

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with the use of force, its limitations, potentialities, and requirements. At the same time, the problems of foreign policy remain whole ones, impossible to slice so as to exclude one kind of consideration or the other, and an investigator continues to have the obligation of following his problem wherever it leads, over any number of artificial and arbitrary boundaries. In the far-off world of 1939, E. H. Carr remarked that if every prospective writer on international affairs had taken a compulsory course in elementary strategy, reams of nonsense would have remained unwritten. The differences today are that there are no books available for such a course and that to understand some of the implications of a subject changing so rapidly and profoundly the prospective writer may have to work them out himself.

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THE NEW LOOK IN INTERNATIONAL JURISPRUDENCE

By P. E. CORBETT

Charles De Visscher, *Théories et Réalités en Droit International Public*, Paris, Editions A. Pedone, 1953, 467 pp.

Julius Stone, *Legal Controls of International Conflict: A Treatise on the Dynamics of Disputes- and War-Law*, New York, Rinehart & Company, Inc., 1954, 851 pp. $12.00.

THE guild of international lawyers—it would be a little sacrilegious still to call it a trades union—wears a tough armor of respectability. Some of its humbler members have long had a suspicion that all was not well in their world; but the high brass remained unshaken in its conviction of moral rectitude and practical wisdom. If vulgar men and turbulent events seemed contemptuous of the legal mystery, this was a temporary outbreak of original sin and ignorance. We were in a time of troubles; but in due course the nations would learn again that salvation lay in a return to the lawfulness of the Middle Ages and the nineteenth century. If any changes were needed to make the international code the adequate guarantee of world order, the positivism of the last hundred years might perhaps be tempered by a new infusion of natural law. There could be no harm in drawing upon that respectable repository of the eternal verities. But let not the pure stream of jurisprudence be defiled with sociological trash.

Professors Charles De Visscher and Julius Stone have done as much as two men could to shake this complacency. From the point of view of the malcontents, it was rare good fortune that so strong a lead toward a new international jurisprudence should be given by a former judge of the Permanent Court of International Justice and one-time President of the International Law Institute, and that his Belgian labors should be reinforced in Australia by the author of 1950's great contribution to legal science, *The Province and Function of Law*.

How near these two writers have come to shaking off the tradi-

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tional assumptions may be gathered from a glance at two or three passages from each.[1]

> The classical doctrine of the law of nations [says De Visscher] is based on the postulate of an international society which defines and subordinates sovereignties for the common good of men. . . . Of these reasonings, as of all those built upon the primacy of the *civitas maxima* or the rationally necessary interdependence of society and law, it may be said that they take as proven precisely what requires proof (p. 116).

> On the plane of reason, men do not deny the existence of supranational values; when it comes to action they rarely bow to any but national imperatives (p. 120).

> The international community is an order in the minds of men; it does not correspond to an established order. It falls short of a legal community in that it lacks the control of force by law (p. 128).

> Psychologically [says Stone], the existence of "a system of law" presupposes, besides naked power, the existence of a community whose members' socio-ethical conventions, and their dependent norms, have to a degree come together. It also presupposes a certain stability of that concurrence, legal justice being both bolstered and checked by legal certainty and uniformity. International law seems to fall short on both heads.
>
> The former deficiency, indeed, . . . that of common conviction—may be increasing. Modern inter-State relations show an increasing "nationalization" of the values of both truth and justice. . . .
>
> However it be as to more limited orders, that of international law as a world system is still, on critical questions, "law in the books" rather than "law in action" (pp. 36-37).

> It is at least probable that the magic circle of the unsolved classical problems will not be broken until we cease to assume that the categories, conceptions and methods of municipal law are sufficient, or even necessarily relevant, either for testing the validity of international law or for understanding its actual operation (p. iv).

Both authors thus set out on their work denying the existence on a general international plane of what has commonly been regarded as the necessary basis of any legal system. One of them explicitly queries even the relevance, in this region of thought, of techniques and categories borrowed from the national sphere. Yet both proceed to analyze and describe the phenomena of world politics by methods and with a terminology derived from national law and committing them to recognition of an operative law of nations. To be quite consistent, they would have had to devise a method and find a language which would have enabled them

[1] The translations in this article are mine.

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to place the patterns observed in the relations of States at different points along a scale stretching from no law at one end to law at the other. As it is, they continue to classify as law institutions that have, on their own premises, only a doubtful claim to that description.

De Visscher's plan is to examine the rules of international law as influenced by the present conditions in the relations of States and particularly by the actual distribution of power between nations. This, he thinks, will exhibit in their reality and flexibility the balances of interest of which these rules are the formal expression. His focus is what he calls the *application* or *operation* (*mise en œuvre*) as distinguished from the *elaboration* of legal rules (pp. 201-2). This is essentially identical with the distinction, made use of by Stone, between "law in the books" and "law in action"—a dichotomy familiar to all students of American jurisprudence. It is not, in my opinion, a useful bit of conceptual apparatus. If, as De Visscher holds (pp. 163-71), efficacy is an essential quality of law, surely the only law is what can be observed in operation. Using a terminology which implies two kinds of law, he, like Stone, seems to me to reinforce one of the existing obstacles to the growth of an effective international order, namely, the confusion between juristic notions of what the law is or might be and the patterns regarded as obligatory and actually observed in the interaction of governments, or, to put it in the simplest form, between the literature about law and law itself. By using the same term, "law" or "droit," for the two things which they believe it essential to distinguish, they take the edge off their own effort.

Neither rigorously consistent method nor anything approaching unambiguous terminology, then, is to be found in these books. Possibly the majority of readers will weep no tears over the fact that the task of formulating a systematic methodology for a sociological jurisprudence in the international field has been left to writers yet to come. What can be confidently recorded is that De Visscher and Stone have put new heart into the movement that would make jurisprudence one of an interdependent and cooperating group of social studies and thus fit it, among other things, for a substantial part in the construction of a

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serviceable world order. Both enjoy high authority, and their insistence on the need for this juristic revolution is in itself an appreciable step forward. But they have done far more than lend their authority. De Visscher has drawn on a rare combination of knowledge and experience, and Stone on amazingly encyclopedic learning, to demonstrate by its works the inadequacy, as a solvent of conflict, of the formalistic jurisprudence so characteristic of the literature of international law. Having thus cleared the ground of its luxuriant growth of illusions, they move forward into an area hitherto regarded as out of bounds for the science of law and make bold to reveal the nature, complexity, and obstinacy of the main forces at work in the international arena. Along the way, De Visscher, who is more explicit on this point than Stone, indicates the additions needed in the professional equipment of the jurist if he is to provide any guidance to the control of these forces. Stone, though he also demonstrates a new jurisprudence in action, is more concerned with the meticulous formulation of detailed rules as they operate in practice, and with accounting for contrasts between these and the texts.

Constant attention to the relation between legal rules and social facts does not make either author forget that the laws with which he is concerned are normative propositions, not mere generalizations from observed conduct (Stone, pp. 35, 39; De Visscher, pp. 165, 170). Far from demanding a total concordance of rule and practice, both recognize the inevitability of a "tension" between law and social facts. This is not the tension which they see between "law in the books" and "law in action," but the measure of discrepancy between even law in action and actual human behavior. The question is whether they are not too tolerant of tension. Their own books reveal clearly enough that, outside a margin of routine business not in the first order of importance, there are few patterns in their field of observation that pass any really searching test of efficacy. But perhaps an optimistic tendency to overlook discrepancies between precept and behavior is necessary to keep serious legal scholars working in this field. There are at any rate excellent reasons for forgiving these two if they continue to classify as law what their own

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conscientious recognition of reality shows to be so frail. Among these reasons, one is especially relevant here. As they examine, in the interaction of governments, what De Visscher so suggestively names the convergences and tensions of law and politics, they show clearly the conjuncture of circumstances that enables any alleged rule actually to exercise its normative influence. But they also reveal in equally high relief, and with a moving combination of knowledge and compassion, the human forces that so often make the best-laid rule an empty formula. No illusions are left about the difficulties that face the would-be legislator for the aggregate of States. If both writers are idealists, theirs is an idealism of purpose, not an idealization of rebellious facts.

De Visscher disavows any purpose of presenting a general treatise embodying all the rules. He selects only the most important institutions and activities from the vast complex of inter-State relations and analyzes them as instances of the interplay of legal notions and political interests (p. 202). His dominant purpose is to formulate a new juristic approach. His book makes a unified and powerful impact.

Stone has a broader target and his effect is more diffuse. True, even he drastically limits his field, concentrating upon disputes, neutrality and war. But this concentration is preceded by ninety pages of general theory and interspersed with "discourses" which repeatedly raise questions relevant to the possibility and entire content of a working law of nations. The author himself hopes that his book may serve as "a comprehensive treatise on the law of disputes, war and neutrality," a textbook for undergraduate and advanced seminar courses, and "a useful starting-point for research in practical problems which arise in the offices of legal advisers, government departments, civil and military and consular and diplomatic missions" (pp. viii-ix). Unfortunately, this multiplicity of audiences results at times in a plethora of detail which might well have been left in its original texts. Again, Stone's strictures on Kelsen's "concentration on textual legalities" might readily be turned against himself. His interpretations of the United Nations Charter and consequent inferences as to the rights and duties of the Organization and its members are subtle enough to delight the most abstract legal logician. Lawyers may

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count it one of the merits of his book that it shows a virtuosity of legal exegesis equal to Kelsen's and leading often to opposite results. Yet, for many readers, the best of this part of his work will be the analysis of procedural developments in United Nations organs that render some of his neatest constructions irrelevant for practical purposes.

Stone relies hardly less than De Visscher on the ideas of the just and the good for the eventual development of an effective law of nations. There are many passages in his text which remind one of De Visscher's statement that

> Between States as within the State, law is a product of morals insofar as the idea of the just, which is its specific content, is inseparable from the idea of the good, which is a moral idea. . . . There is only one possible moral and legal justification for an obligatory international law. It is to be found in the return to man, in tying the conception of the State, organization and means, to the person who is its end (pp. 127-28).

This is surely identical in substance with Stone's conviction that a system of law presupposes "psychologically" a concurrence of "socio-ethical conventions and their dependent norms." The same judgment is implied in the deep concern with which he records the "depersonalization of international relations and the increasing 'nationalization' of the values of both truth and justice" (pp. 36-37).

It would be hard to say which author realizes more sharply the remoteness of a universal community tied together by shared ideas of the just and the good. De Visscher finds charitable excuse for the formalism of the traditional and still prevailing juristic approach in the exceptional stability of nineteenth-century international relations and the Christian-European heritage (then still unsquandered) of a community of thought and moral aspiration. The violent commotions of our time have swept away that stability, while new political ideologies, and the eruption on the international scene of peoples of diverse civilizations have shattered the European community of values. Unity survives only in the books. "Doctrine," he concludes, "can better serve the progress of law by pointing out the consequences, sometimes frankly anti-social, of the present distribution of power than by abandoning itself to a sort of 'judicial totalitarianism' which throws up a

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façade of unreal architecture before the actual disorder of international relations" (pp. 168-71).

For Stone, the greatest difficulty appears to lie in the "interposition of State entities between the human beings, citizens of the several States," with its resulting displacement of personal ideas of the just and the good. In a "sociology of international law," which to him means the study of the interaction of international rules and human behavior, he would assign priority to more knowledge about the changes in human attitudes which this interposition has produced.[2] De Visscher is equally concerned about the "depersonalization" that has accompanied the increasing concentration of powers in the State. But if these phenomena demand the jurist's study, so does one alleviating factor which is curiously missing in the calculus of hope to which both writers cling. It may be possible to demonstrate that the State's growing direction of international enterprise and communication, together with such technological innovations as weapons of mass destruction and remote control, have brought about some lapse of personal contact and some atrophy of human sympathy and responsibility. But it can probably also be shown that this loss has been partially, if not wholly, offset by a voluminous if fleeting interchange of visitors, by an increasing perception of the interdependence of welfares and a greater readiness to expend national resources for the relief of foreign distress. When Stone complains of "the progressive breakdown" of human communication (pp. xlii-xliii), one remembers how groups have kept in touch and maintained fruitful cooperation across the most hostile frontiers. Nor is the complaint quite consistent with his statement of the negative effects of "the multiplication of inter-cultural contacts by modern rapid communications" (p. 35). He cannot have it both ways.

De Visscher, for all his realism, remains optimistic enough to find some elements of law moderating the political forces at work in the international arena. But the possibility of development into a legal system depends, for him, on the progress of international organization. The man of law must therefore join the

[2] See, e.g., pp. liv-lv and 37-49.

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man of politics in this creative work, which consists essentially in imposing limitations upon existing sovereignties in the higher interest of mankind. To fit himself for the task, he must recognize the insufficiencies of his own discipline, cease exaggerating the separation of law and morals in the international field, and welcome the enrichment of jurisprudence by sociology.

There is no future, in De Visscher's opinion, in rules of war and neutrality. From one who has found as much law as he does in the "convergences and tensions of law and politics" among States, this comes as something of a surprise. It is the point where he parts company with Stone, who has made the adaptation of the rules of war and neutrality to new political doctrine and structure and new military techniques the central object of his present work. Here, after long doubt, I find myself on Stone's side. In the main, as De Visscher insists, the future of international law is one with that of international organization. But obviously there is no present power in that quarter to prevent armed conflict. Stone, I find, has made the most balanced, yet most persuasive, case in recent literature for continuing an effort which to our certain knowledge has preserved some measure and some human kindness in the massive, mechanized havoc that is contemporary war.

It must be admitted, however, that when it comes to the greatest contemporary problem in his field, Stone can find no solution in law. The only institution which could, in his judgment, ensure that atomic weapons will not be used would be a supranational "State-like political organization" which "is precisely what the political realities of present international life rule out." His examination of the factors that prevent any agreement proscribing these weapons is an exhibition of his broader jurisprudence at its best. True, in the end he can counsel nothing but that men, knowing the destructive potentialities of the new arms, should not urge their values with impatient zeal (pp. 345-48). In other words, he despairs of a legal rule of war precisely where effective restraint is now most desperately needed, and must fall back upon the hope that a new magnitude of fear will do the work of law. I have no quarrel with this conclusion. It may be worth some emphasis, however, that on this point at least he is perforce in agreement with De Visscher's judgment that organiza-

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tion is the condition of progress toward an effective law of nations. The point is, moreover, one of far-reaching importance, because failure of control here may rob all the existing shreds of law that Stone so cherishes even of their present limited utility.

De Visscher and Stone are both lawyers, and both devoted to turning their vocation to the greatest social good. They are also unusually endowed observers of the human scene. They would liberate jurisprudence from a formalism which has rejected as irrelevant the testing of legal theory by the realities of international life, and make of it a frankly policy-making art. The potentialities of law in the relations of States, emerging from their unsparing scrutiny, are small now and promise no rapid expansion. These books are dedicated to the long pull. Their lesson is a severe one for jurists prone to attribute total competence to the legal technique. Yet the effect is also positive. Having taken the measure of law as a mode of international control, these teachers continue to find, in the world-wide interplay of particularist demands of power on the one side and general rules of law and ethics on the other, opportunities for the promotion of order and of their conceptions of the just. Their books have at least begun to show how these opportunities may be recognized and exploited.

In a review of Mr. K. M. Panikkar's *Asia and Western Dominance* which appeared in the October 1954 issue of *World Politics,* Mr. Guy Pauker quoted from R. Hutheesing's book, *The Great Peace: An Asian's Candid Report on Red China.* The quotation, attributed to Mr. Nehru, stated: "He will be a Communist in Peking and a champion of freedom in Washington so long as it takes Mr. Panikkar somewhere." The editors of *World Politics* have now learned of a press release of June 6, 1954, by the Prime Minister's secretariat, in which Mr. Nehru explicitly denies ever having made this remark.

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